The American Political Science Review

Articles and Comments by Raymond E. Wolfinger, Frederick W. Frey, Donald VanDeVeer, Felix E. Oppenheim, Lawrence W. Beer, Howard D. Hamilton, Peter C. Ordeshook, Martin Shubik

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Evron M. Kirkpatrick

Toward a More Responsible Two-Party System: Political Science, Policy Science, or Pseudo-Science?

Ronald Inglehart

The Silent Revolution in Europe: Intergenerational Change in Post-Industrial Societies

Robert S. Erikson

The Electoral Impact of Congressional Roll Call Voting

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The Development of Policy Thinking in Adolescence

James Q. Wilson and Edward C. Banfield

Political Ethos Revisited

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CONTENTS

960 Editorial Comment

965 Toward a More Responsible Two-Party System: Political Science, Policy Science, or Pseudo-Science? Evron M. Kirkpatrick

991 The Silent Revolution in Europe: Intergenerational Change in Post-Industrial Societies Ronald Inglehart

1018 The Electoral Impact of Congressional Roll Call Voting Robert S. Erikson

1033 The Development of Policy Thinking in Adolescence Richard M. Merelman

1048 Political Ethos Revisited James Q. Wilson and Edward C. Banfield

1063 Nondecisions and the Study of Local Politics Raymond E. Wolfinger

1081 Comment Frederick W. Frey

1102 Rejoinder Raymond E. Wolfinger

1105 Oppenheim's Defense of Noncognitivism Donald VanDeVeer

1115 Comment Felix E. Oppenheim

1117 Rejoinder Donald VanDeVeer

1119 Freedom of Information and the Evidentiary Use of Film in Japan: Law and Sociopolitics in an East Asian Democracy Lawrence W. Beer

1135 The Municipal Voter: Voting and Nonvoting in City Elections Howard D. Hamilton

1141 Pareto Optimality in Electoral Competition Peter C. Ordeshook

1146 Comment Martin Shubik

1146 Rejoinder Peter C. Ordeshook

1147 Communications

1158 Book Reviews

1262 Index to Volume LXV

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ARTICLES

965 **Toward a More Responsible Two-Party System: Political Science, Policy Science, or Pseudo-Science?** The 1950 Report of the APSA Committee on Political Parties, "Toward a More Responsible Two-Party System," is relevant today to current problems of public policy and party reform and to the efforts of political scientists, as political scientists, to contribute to the resolution of these problems. This essay examines the Report from a policy science perspective.

The Report was explicitly therapeutic in aim. It defined health, diagnosed ills, and prescribed remedies for the American party system; through the remedies prescribed, the whole American political system was to be restored to health. The healthy democratic system was asserted to be one in which the two national parties were cohesive, disciplined, programmatic, and responsible; internally responsible to their members through primaries, caucuses and conventions, and externally responsible to the whole electorate for carrying out their programs. The programs of the two parties were to be clearly differentiated so as to provide the electorate a real choice. The ills of the Ameican system were said to be due to the failure of parties to have these characteristics. The prescription was recommendation for comprehensive reform.

Despite the special expertise of political scientists on such "constitutional" questions and the work of such distinguished predecessors as Wilson, Goodnow, Lowell, Ford, and Herring, the Report was both normatively and empirically deficient. Little attempt was made to clarify or justify norms or goals. Repeatedly, instrumental propositions linking proposed reforms to goals were based on inadequate evidence or no evidence at all. Even in 1950, evidence (not mentioned in the Report) was available that cast doubt on the Committee's description of the political world. Subsequent research has produced a rich body of literature making clear that much of the substance of the Report is simply mistaken.

The errors of the Report do not vitiate its goals; democratic potential is not revealed by democratic practices. But the errors drastically affect the utility of the Report as policy science. The failure of the Report as policy science is due, in part, to failures of the discipline to clarify the roles of political scientist as policy scientist, to explore adequately the problems of relating knowledge to goals, to pay appropriate attention to the development of political theory, and to develop intellectual tools more specifically suited to the tasks of policy science. The last half of the essay is devoted to an examination of these problems, concluding that the political scientist will succeed in being effective in the policy field just to the extent he succeeds at his own distinctive tasks, in sharpening his own tools, and in thoughtfully applying his special knowledge and skills.

By EVRON M. KIRKPATRICK, Executive Director, The American Political Science Association.

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991 **The Silent Revolution in Europe: Intergenerational Change in Post-Industrial Societies.** A transformation of basic political priorities may be taking place in Western Europe. I hypothesize: (1) that people have a variety of needs which are given high or low priority according to their degree of fulfillment: people act on behalf of their most important unsatisfied need, giving relatively little attention to needs already satisfied—except that (2) people tend to retain the value priorities adopted in their formative years throughout adult life. In contemporary Western Europe, needs for physical safety and economic security are relatively well satisfied for an unprecedentedly large share of the population. Younger, more affluent groups have been formed entirely under these conditions, and seem relatively likely to give top priority to fulfillment of needs which remain secondary to the older and less affluent majority of the population. Needs for belonging and intellectual and esthetic self-fulfillment (characterized as "post-bourgeois" values) may take top priorities among the former group. Survey data from six countries indicate that the value priorities of the more affluent postwar group do contrast with those of groups raised under conditions of lesser economic and physical security. National patterns of value priorities correspond to the given nation's economic history, moreover, suggesting that the age-group differences reflect the persistence of preadult experiences, rather than life cycle effects. The distinctive value priorities imply distinctive political behavior—being empirically linked with preferences for specific political issues and political parties in a predictable fashion. If the respective age cohorts retain their present value priorities, we would expect long-term shifts in the political goals and patterns of political partisanship prevailing in these societies.

By RONALD INGLEHART, Assistant Professor of Political Science, University of Michigan and University of Geneva.

1018 **The Electoral Impact of Congressional Roll Call voting.** This paper presents evidence that candidate issue positions have a measurable impact on elections for the U.S. House of Representatives. For eight election years, electoral margins of Northern incumbent congressional candidates were examined to test the proposition that "moderates" within each party are better vote getters than those whose roll call records reflect their party's ideological extreme. The effects of roll call positions on election results were estimated by examining the relationships between roll call "extremism" and vote margins with district presidential voting held constant as a control for normal constituency voting habits. Although no strong support was found for the proposition that Democratic Representatives lose electoral support when they take extremely liberal roll call positions, a clear pattern emerged for Republicans: the Republican Congressmen who are the best vote getters tend to be the relative moderates and liberals who avoid the extreme conservative end of the political spectrum. An analysis of survey data suggests that the small group of voters whose electoral decisions are influenced by their Republican Congressman's roll call performance are found within the ranks of a select group who are both free of strong partisan motivations and highly politically informed.

By ROBERT S. ERIKSON, Assistant Professor of Government, Florida State University.

1033 **The Development of Policy Thinking in Adolescence.** This article identifies four fundamental modes of thought employed in the cognition of policy problems. These modes of thought are moral, cause-effect, sociocentric, and imaginative. Nine variants of these four forms are described and investigated among a small sample of adolescents. The maturation of these forms of thought appears limited during adolescence, and change is not well predicted by the respondent's level of politicization. The article concludes with some speculations about the structure of socialization theory as it relates to the development of fundamental forms of political thinking.

By RICHARD M. MERELMAN, Associate Professor of Political Science, University of Wisconsin.

1048 **Political Ethos Revisited.** An effort to test the existence and correlates of the "unitarist" and "individualist" political ethos (first discussed in *City Politics* under the labels "middle-class Anglo-Saxon ethos" and "immigrant ethos") in a sample of 1,059 mostly male Boston homeowners reveals that about one fifth of the respondents have one or the other ethos when defined by two sets of attitudes and about one eighth have one or the other when defined by three sets of attitudes. In general, the respondents displaying each attitude or two or more attitudes in the predicted combinations have the predicted ethnic, religious, income, and educational attributes. Jewish voters, however, are less likely than predicted to have the good government attitude, whereas Irish and Polish respondents are more likely to have it. Upper-income Yankees were strongly unitarist as defined by all three attitudes.

By JAMES Q. WILSON, Professor of Government, Harvard University and EDWARD C. BANFIELD, Henry Lee Shattuck Professor of Urban Government, Harvard University.

1063 **Nondecisions and the Study of Local Politics.** The widely cited concept of nondecisions is particularly prominent in criticisms of "pluralist" research on local politics. But no scholars, including those who introduced the notion of nondecisions, have done empirical research explicitly on this topic. The lack of research on nondecisions reflects the concept's weaknesses as a guide to field research: (1) It involves a number of unrealistic assumptions about political life. (2) It requires data that are difficult to gather or wholly unobtainable. (3) Even the data that can be collected do not provide a basis for sensible conclusions about the distribution of political power.

The impracticability of research on nondecisions is not a serious setback for political science, however, for most of its specific component ideas, such as the policy consequences of different governmental forms or the impact of political socialization, are being studied without reference to the notion of nondecisions. Judged by its utility for empirical research, then, the idea of nondecisions appears to be superfluous. The same might be said of the notion of "power structures."

By RAYMOND E. WOLFINGER, Professor of Political Science, University of California, Berkeley.

Comment. By FREDERICK W. FREY, Professor of Political Science, Massachusetts Institute of Technology.

Rejoinder. By RAYMOND E. WOLFINGER.

1105 **Oppenheim's Defense of Noncognitivism.** In his recent book, *Moral Principles in Political Philosophy*, Felix Oppenheim provides a useful examination of the assumptions of well known figures in the history of political philosophy concerning the logical status of moral principles. Classifying them as cognitivists (if they view fundamental moral principles as either true or false) or noncognitivists (if they view moral principles as neither true nor false), Oppenheim attempts to exhibit the inadequacy of the cognitivist point of view and, importantly, the adequacy of the noncognitivist position. My critique aims at demonstrating the inconclusiveness of Oppenheim's arguments against cognitivism. Oppenheim presupposes the availability of a plausible and workable criterion for determining when a sentence counts as a statement (statements are for Oppenheim entities which are true or false and, thus, "cognitively meaningful"), but he fails to provide any attractive candidate for that position. Further critical discussion revolves around the following related questions: Is there adequate positive support for the noncognitivist view? Does it allow for the rationality of fundamental moral commitments? And can Oppenheim really justify his case that, far from being irrational or pernicious, noncognitivism is naturally associated with certain humanistic ideals, such as toleration of those of differing moral and political viewpoint?

By DONALD VANDEVEER, Assistant Professor of Philosophy, North Carolina State University.

Comment. By FELIX E. OPPENHEIM, Professor of Government, University of Massachusetts, Amherst.

Rejoinder. By DONALD VANDEVEER.

1119 **Freedom of Information and the Evidentiary Use of Film in Japan: Law and Sociopolitics in an East Asian Democracy.** This study sets forth the post-1945 development and present status of Japanese constitutional and procedural law on court-mass media relations, while analyzing aspects of the interaction between law and sociopolitical thought and behavior. A recent and dramatic illustration of the issues is provided by the Hakata Station Film Case: A Fukuoka court's *subpoena* (August 29, 1969) for newsfilm taken during a student-police encounter occasioned conflict between Japan's mass media and courts; the dispute was resolved by a film seizure (March 4, 1970) three months after the Supreme Court had upheld the *subpoena's* constitutionality. The media maintain that Article 21 of the Constitution (freedom of expression) gives them the right to determine when their used or unused television film or still photographs may be employed as court evidence, even in the absence of privileged communications. This and other court cases considered, arising from Japan's perennial demonstrations, illustrate a strong tendency toward in-group unanimity, new problems in news and evidence gathering, and the operation of a non-Western legal system influenced by Japanese, European, and American traditions.

By LAWRENCE W. BEER, Associate Professor of Political Science and Director, Center for East Asian Studies, University of Colorado, Boulder.

1135 **The Municipal Voter: Voting and Nonvoting in City Elections.** The few case studies of participation in local elections display distinct patterns, but the measurements lack comparability with each other or with national election voting studies. By application of the methodology, variables, and categories of the presidential election studies to a Toledo city election, the composition of the electorate is compared with that in presidential elections and some sharp contrasts which appear to have significant implications are observed. Some data calculated from Merriam and Gosnell's classic *Non-Voting* reveal some developments since 1923. From two prominent characteristics of municipal elections, nonpartisan form and low turnout, a few hypotheses about participation in city elections are deduced and examined. The data support the general proposition that most of the psychological, demographic, and socioeconomic variables display comparable amplitudes in city and presidential elections, but their significance is much greater in city elections, because the low voting level makes their impact proportionately greater.

By HOWARD D. HAMILTON, Professor of Political Science, Kent State University.

1141 **Pareto Optimality in Electoral Competition.** The core of welfare economics consists of the proof that, for certain classes of goods, perfectly competitive markets are efficient in that they provide Pareto optimal allocations of these goods. In this paper, the efficiency of competitive elections is examined. Elections are modeled as two-candidate zero-sum games, and three kinds of equilibria for such games are identified: pure, risky, and mixed strategies. It is shown, however, that regardless of which kind of equilibrium prevails, if candidates adopt equilibrium strategies, an election is efficient in the sense that the candidates advocate Pareto optimal policies. But one caveat to this analysis is that while an election is Pareto optimal, citizens can unanimously prefer markets to elections as a mechanism for selecting future policies.

By PETER C. ORDESHOOK, Associate Professor of Political Science, Carnegie-Mellon University.

Comment. By MARTIN SHUBIK, Professor of the Economics of Organization, Yale University.

Rejoinder. By PETER C. ORDESHOOK.

1147 COMMUNICATIONS

From David Seidman, Harlan Hahn, Leon Hurwitz, Michael Taylor and V. M. Herman, Walter Dean Burnham, Jerrold G. Rusk.

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960 EDITORIAL COMMENT

BOOK REVIEWS

1158 Political Theory, History of Political Thought and Methodology

Abt, *Serious Games;* AND Barton, *A Primer on Simulation and Gaming.* Charles D. Elder, p. 1158
Bendix, *Embattled Reason, Essays on Social Knowledge.* Scott Greer, p. 1159
Blalock, *Theory Construction, from Verbal to Mathematical Formulations.* Charles F. Cnudde, p. 1161
de Crespigny and Wertheimer, eds., *Contemporary Political Theory.* Wilfrid E. Rumble, p. 1162
Fain, *Between Philosophy and History: The Resurrection of Speculative Philosophy of History Within the Analytic Tradition.* Louis O. Mink, p. 1164
Janda, *Data Processing: Applications to Political Research.* Charles Lewis Taylor, p. 1165
Nettler, *Explanations.* Robert Brown, p. 1167
Richter, *Essays in Theory and History: An Approach to the Social Sciences.* Peter Bachrach, p. 1168
Rogow, ed., *Politics, Personality, and Social Science in the Twentieth Century: Essays in Honor of Harold D. Lasswell.* Robert H. Horwitz, p. 1169
Strayer, *On the Medieval Origins of the Modern State.* Antony Black, p. 1171
Thompson, *The Democratic Citizen: Social Science and Democratic Theory in the Twentieth Century.* John D. Lewis, p. 1172

1174 American Government and Politics

Banovetz, ed., *Managing the Modern City.* Arthur Naftalin, p. 1174
Blumberg, *Industrial Democracy: The Sociology of Participation;* and Derber, *The American Idea of Industrial Democracy, 1865–1965.* G. David Garson, p. 1175
Canty, *A Single Society: Alternatives to Urban Apartheid;* and Tabb, *The Political Economy of the Black Ghetto.* Morris Levitt, p. 1176
Chittick, *State Department, Press, and Pressure Groups: A Role Analysis.* John W. Outland, p. 1178
Davies, *Politics of Pollution.* Ronald O. Loveridge, p. 1179
Draper, *The Rediscovery of Black Nationalism.* Robert E. Martin, p. 1180
Dexter, *The Sociology and Politics of Congress.* Robert L. Peabody, p. 1182
Goldman and Jahnige, *The Federal Courts as a Political System.* C. Herman Pritchett, p. 1183
Horn, *Unused Power.* Barbara Hinckley, p. 1185
Kohlmeier, *The Regulators: Watchdog Agencies and the Public Interest.* David M. Welborn, p. 1186
Kovel, *White Racism: A Psychohistory;* and Friedman, *The White Savage: Racial Fantasies in the Postbellum South.* Donald G. Baker, p. 1187
McClellan, *The Heathen Chinee, A Study of American Attitudes Toward China, 1890–1905.* Harold R. Isaacs, p. 1189
McLennan, ed., *Crime in Urban Society.* Robert C. Jacobs, p. 1189
Miller, *The Supreme Court and the Uses of History.* David Farrelly, p. 1190
Nelli, *The Italians in Chicago 1880–1930: A Study in Ethnic Mobility.* Edgar Litt, p. 1191
Rogin and Shover, *Political Change in California: Critical Elections and Social Movements, 1890–1966.* John A. Vieg, p. 1192
Welch, *American Images of Soviet Foreign Policy.* Robert G. Wesson, p. 1193
Wirt, *Politics of Southern Equality: Law and Social Change in a Mississippi County.* Robert J. Steamer, p. 1194

1196 Comparative Government and Politics

Anderson, *The Political Economy of Modern Spain: Policy-Making in an Authoritarian System.* Joan C. Ullman, p. 1196
Brunelli, et al., *La Presenza Sociale del PCI è della DC.* Alan J. Stern, p. 1197
Burnett, *Political Groups in Chile: The Dialogue Between Order and Change.* Francisco José Moreno, p. 1198
Cartwright, *Politics in Sierra Leone 1947–1967.* Clement Cottingham, p. 1199
Ch'en, ed., *Mao Papers: Anthology and Bibliography.* Richard Baum, p. 1200
Economic Commission for Latin America, *Development Problems in Latin America.* Gayle Avant, p. 1203
Fischer, ed., *Science and Ideology in the Soviet Union.* Eugene Rabinowitch, p. 1204
Fischer, ed., *The Social Sciences and The Comparative Study of Educational Systems.* Byron G. Massialas, p. 1206

Fryde and Miller, eds., *Historical Studies of the English Parliament.* John Clarke Adams, p. 1208
Goldsworthy, *Colonial Issues in British Politics 1945–1961.* Trevor Lloyd, p. 1208
Guillain, *The Japanese Challenge;* AND Kahn, *The Emerging Japanese Superstate, Challenge and Response.* Hans H. Baerwald, p. 1209
Horecky, *East Central Europe: A Guide to Basic Publications.* George Klein, p. 1211
Humes and Martin, *The Structure of Local Government;* AND Leemans, *Changing Patterns of Local Government.* Francine F. Rabinovitz, p. 1212
Joint Development Group, *The Postwar Development of the Republic of Vietnam: Policies and Programs.* Allan E. Goodman, p. 1213
Kim, *Modern Korea: The Socialist North, Revolutionary Perspectives in the South, and Unification.* John C. H. Oh, p. 1215
Kuper, *Kalahari Village Politics: An African Democracy.* Ronald Cohen, p. 1216
Lean, *The Napoleonists: A Study in Political Disaffection.* Nannerl O. Keohane, p. 1217
Liang, *The Berlin Police Force in the Weimar Republic.* Ronald F. Bunn, p. 1218
Lieber, *British Politics and European Unity: Parties, Elites and Pressure Groups,* D. J. Wenden, p. 1219
Liebman, *The Politics of Puerto Rican University Students.* Leila A. Sussmann, p. 1220
Mörner, ed., *Race and Class in Latin America;* AND Vallier, *Catholicism, Social Control, and Modernization in Latin America.* Kalman H. Silvert, p. 1221
Nakane, *Japanese Society.* Tosh Lee, p. 1223
O'Brien, *The Mourides of Senegal; The Political and Economic Organization of an Islamic Brotherhood.* Lucy C. Behrman, p. 1202
Osborn, *Soviet Social Policies: Welfare, Equality and Community.* David S. Lane, p. 1224
Parry, *Political Elites.* Robert Presthus, p. 1225
Prybyla, *The Political Economy of Communist China.* Yuan-li Wu, p. 1226
Rahul, *The Himalaya Borderland.* Kunjar M. Sharma, p. 1228
Rintala, *Four Finns: Political Profiles.* John H. Hodgson, p. 1228
Schreiber, *U.S. Corporate Investments in Taiwan.* Alek A. Rozental, p. 1229
Smith, *Heinrich Himmler—A Nazi in the Making, 1900–1926.* Raul Hilberg, p. 1230
Stern, *The Process of Opposition in India.* Stanley A. Kochanek, p. 1231
Thomson, *No Exit From Vietnam.* Edward R. Wright, p. 1232
Toma, ed., *The Changing Face of Communism in Eastern Europe.* R. Barry Farrell, p. 1234
Volin, *A Century of Russian Agriculture: From Alexander II to Khrushchev.* Sidney I. Ploss, p. 1235
Weidner, ed., *Development Administration in Asia.* John T. Dorsey, p. 1237
Wheeler, *The Politics of Pakistan: A Constitutional Quest.* George M. Platt, p. 1238
White, *The Sōkagakkai and Mass Society.* James Allen Dator, p. 1240
Willis, *Italy Chooses Europe.* Norman Kogan, p. 1241

1242 International Politics, Law and Organization

Andrassy, *International Law and the Resources of the Sea.* Stanley S. Jados, p. 1242
Bailey, *Voting in the Security Council.* David A. Kay, p. 1243
Barnaby, *Preventing the Spread of Nuclear Weapons.* Ciro E. Zoppo, p. 1243
Besson, *Die Aussenpolitik der Bundesrepublik: Erfahrungen und Masstäbe;* AND Schmidt, *Strategie des Gleichgewichts: Deutsche Friedenspolitik und die Weltmächte.* Joachim Hütter, p. 1245
Cohen, *International Monetary Reform, 1964–69.* Henry Simon Bloch, p. 1246
Elliott-Bateman, ed., *The Fourth Dimension of Warfare.* Harry Howe Ransom, p. 1248
Feis, *From Trust to Terror: The Onset of the Cold War, 1945–1950.* Paul Seabury, p. 1249
Hartmann, *The New Age of American Foreign Policy.* David V. Edwards, p. 1249
Levinson and de Onís, *The Alliance that Lost Its Way: A Critical Report on the Alliance for Progress.* Michael G. Webster, p. 1250
Miller, *International Community Power Structures: Comparative Studies of Four World Cities.* Robert T. Daland, p. 1251
Quester, *Nuclear Diplomacy, The First Twenty-Five Years.* Simon H. Serfaty, p. 1253
Rakowska-Harmstone, *Russia and Nationalism in Central Asia: the Case of Tadzhikistan.* Owen Lattimore, p. 1254
Shepherd, *Nonaligned Black Africa: An International Subsystem.* Amry Vandenbosch, p. 1255
Shore, *Fact-Finding in the Maintenance of International Peace.* W. M. Reisman, p. 1256
Taylor, *Nuremberg and Vietnam: an American Tragedy.* John H. E. Fried, p. 1257
Wesson, *The Imperial Order.* Kenneth W. Thompson, p. 1258
White, *Decision-Making for Space; Law and Politics in Air, Sea, and Outer Space.* Carl Q. Christol, p. 1260

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EDITORIAL COMMENT

Referees. The managing editor of a scholarly journal is—regardless of his fantasy life—not like William Allan White or Colonel Robert Rutherford McCormick. The scholars who make up the community of students of politics essentially write their own journal. The editor's role is unlike that of a lion of the pressroom, and rather more like that of a social director on a low-cost cruise. He must encourage the timid, rein in the brash, seek out hidden talent, and leave strictly alone those who don't want to be annoyed. Few cruises, and no journals, can survive an overabundance of the last. This journal is fortunate that so many scholars have over the years been willing to contribute their time and energy to maintaining the quality of the *Review*, not only by writing for it, but also by undertaking the anonymous and solitary tasks of reading manuscripts and advising the editor on their suitability for publication.

The sheer number of manuscripts we receive precludes their careful evaluation by any one person. The diversity of political science is enormous. It has been discovered in the past, and it is no less true today, that no single editor can possibly know enough himself to judge the quality of manuscripts over the full range of concerns that political scientists write about. And so we come to rely upon outside evaluations.

Associate editors undertake especially heavy burdens, agreeing in advance to read whenever the managing editor calls on them. In addition, we call upon many other scholars, students of politics who seem to the managing editor especially well qualified in particular instances to answer the questions we must have answered in order to determine the suitability of proffered contributions for *Review* publication. To the readers of specialized manuscripts, we ask: Would this article, if published, be a useful or important contribution to the thinking of those working in the field? To the readers of less specialized manuscripts, we ask: Does this article summarize, codify or display information in a particularly interesting, novel, or comprehensive manner?

Implicit in these questions is the belief that there is room for both kinds of article in the *Review*. We are trying to make the *Review* a forum for scholars who may feel free to communicate with one another in languages and at levels as arcane as may be necessary to advance the frontiers of political knowledge. At the same time the *Review* is, and should be, a meeting place for scholars who while attending to their own work also want to keep abreast of what others are doing, perhaps in order to teach better, in order to stimulate their own scholarly work, in order to understand the world they live in a little better, or perhaps just for the hell of it.

Referees, because of their knowledge of the subject matter, help to keep the commerce in this forum of ours responsive to the most enlightened current thinking in political science. When they turn articles down, they generally give reasons, which we faithfully pass on anonymously to authors. All this service is unpaid and, in the nature of things, unsung. Last year nearly 400 scholars rendered that service to the rest of us in the profession. Some of these referees were distinguished professors at major graduate schools. Others were very junior scholars just starting out. Some were not political scientists at all, but philosophers, lawyers, economists, statisticians, historians, sociologists or psychologists by trade, all with special knowledge in a subfield of our discipline. Most live in this country, but some live in other parts of the world. To this varied, public-spirited company we owe a great debt of thanks.

Nasty Letters. Every once in a while, mail is received in this office the tone of which can best be described as nasty. It is unpleasant to receive such letters, and we would like to urge readers who may have one or more nasty letters stored up and waiting for a target not to send them to the *Review* but rather to some hapless public official who, as we all know, in a democracy is supposed to enjoy abuse.

Readers may suspect, however, inexplicably, that public officials are not responsible for the format of the *Review,* or for the algebra to be found in its pages, or for delays in the evaluation of articles. This will go some distance toward explaining why certain nasty letters come our way.

We deduce this from the telltale fact that most of the nasty letters we get contain a substantive complaint. Fairly often the complaint is justified, and requires an apology or corrective action or both. We believe we could learn from these complaints if they were couched in temperate—even in plaintive—language. What we infer from the annoyed tone of our irate correspondents, regardless of the substantive message, is that some people feel sufficiently diffident about, or distant from, the *Review* to wait for the last straw to break the back of their equanimity before writing in. So we ask our correspondents in future not to wait that long. If we are wrong in our diagnosis, we suppose the net effect will be for us to get more nasty letters sooner. But if we are right, perhaps we can brighten everybody's day a little by offering to try to do something about little annoyances with the *Review* before they become big ones.

Youth. One of the presumed advantages in heavy reliance upon anonymous evaluations of proffered articles is that big shots in the profession must take their chances along with graduate students, and the contents of an article, not who signed it, consequently plays the largest part in influencing the judgment of readers. We assume this works pretty well most of the time. Sometimes, members of the invisible colleges within our discipline are all too visible to one another, and the only way to conceal the identity of an author is to send his manuscript to somebody who is unqualified to read it by virtue of being so far out of touch with his field that he is unaware that Professor Zilch is working a particular well-publicized vineyard. Since an unqualified reader is, we think, worse than one who knows too much, we sometimes concede defeat and take advice from readers even when they claim they can spot who an author is a mile away upwind.

Thus, unavoidably, we believe, a few breaches of anonymity creep into our evaluation process. This does not seem to have led to a take-over of the *Review* by aged notables, however. Consider the roster of article and comment authors whose work appeared in volume 65, of which this is the last issue. There were 62 of them. We invite interested readers to look up their ages in the APSA biographical directory. Since the managing editor is at present trying to recover from a misspent youth we offer herewith a much simpler, and conceivably a less definitive test of the general receptivity of the *Review* to persons who have not yet reached the pinnacle of academic status. Looking at the academic ranks of the 62 authors of volume 65, we discover that 26 authors were identified as assistant professors or lecturers at the time of publication, 16 were associate professors, and only 20 were full professors. We have no idea of the ranks of persons who sent articles in, or who sent in what proportion of submissions, so there may be significant skews in the ranks of authors of works we finally printed, although we doubt it.

These figures can attest at any rate to the proposition that junior members of the profession are getting their work into the *Review,* and this work is taking up a goodly portion of the *Review's* available space. Because this sort of information seems to interest some of our readers, we report it. We neither admire nor deplore the results themselves, however. Asking how old an author is rarely tells us what we want to know about a work of scholarship in political science.

Articles Accepted for Future Publication.

Robert Axelrod, University of California, Berkeley, "Where the Votes Come From:

An Analysis of Electoral Coalitions From 1952–1968"

Gordon S. Black, University of Rochester, "A Theory of Political Ambition: Career Choices and the Role of Structural Incentives"

Richard W. Boyd, Wesleyan University, "Popular Control of Public Policy: A Normal Vote Analysis of the 1968 Election"

Charles S. Bullock, III, University of Georgia, "Freshman Committee Assignments and Reelection in the U.S. House of Representatives" and "House Careerists: Changing Patterns of Longevity and Attrition"

Charles E. Butterworth, University of Maryland, "Averroës: Politics and Opinion"

Thomas W. Casstevens, Oakland University, "The Committee Function"

B. Michael Frolic, York University, "Decision Making in Soviet Cities"

Alexander L. George, Stanford University, "The Case for Multiple Advocacy in Making Foreign Policy"

Arnold Kanter, The Brookings Institution, "Congress and the Defense Budget: 1960–1970"

Isaac Kramnick, Yale University, "The Politics of Political Philosophy, A Case Study: Godwin's Anarchism and Radical England"

René Lemarchand, University of Florida, "Political Clientelism and Ethnicity in Tropical Africa: Competing Solidarities in Nation Building"

Robert J. Lieber, University of California, Davis, "Interest Groups and Foreign Policy: British Entry into Europe"

Eugene F. Miller, University of Georgia, "Positivism, Historicism, and Political Inquiry"

Andre Modigliani, Harvard University, "Hawks and Doves, Isolationism and Political Distrust: An Analysis of Public Opinion on Military Policy"

Edward N. Muller, State University of New York, Stony Brook, "A Test of a Partial Theory of Potential for Political Violence"

Willard A. Mullins, Carleton University, "On the Concept of Ideology in Political Science"

John M. Orbell and Toru Uno, University of Oregon, "A Theory of Neighborhood Problem-Solving: Political Action *vs.* Resident Mobility"

Gerald M. Pomper, Rutgers University, "From Confusion to Clarity: Issues and American Voters, 1956–1968"

Jeffery L. Pressman, Dartmouth College, "Preconditions of Mayoral Leadership"

George F. Quester, Cornell University, "Some Conceptual Problems on Nuclear Proliferation"

Austin Ranney, University of Wisconsin, "Turnout and Representation In Presidential Primary Elections"

Thomas W. Robinson, The Rand Corporation, "The Sino-Soviet Border Dispute: Background, Development, and the March 1969 Clashes"

John Gerard Ruggie, Dotation Carnegie Pour La Paix Intérnationale, "Collective Goods and International Organization: Public Choice as a Focus in the Analysis of International Organization"

David Sankoff, University of Montreal, and Koula Mellos, University of Ottawa, "The Swing Ratio and Game Theory"

James Scott, University of Wisconsin, "Patron-Client Politics and Political Change in Southeast Asia"

Kenneth A. Shepsle, Washington University, "The Strategy of Ambiguity: Uncertainty and Electoral Competition"

Lee Sigelman, Vanderbilt University, "Administrative Development and Overparticipation: A Test of the Imbalance Thesis"

H. Michael Stevenson and Donald G. Morrison, York University, "Integration and Instability: Patterns of African Political Development"

Peter Willetts, Makerere University College, "Cluster-Bloc Analysis and Statistical Inference"

"Toward A More Responsible Two-Party System": Political Science, Policy Science, or Pseudo-Science?*

EVRON M. KIRKPATRICK

The American Political Science Association

From 1946–1950 I was a member of The American Political Science Association's Committee on Political Parties. This Committee gave birth to a remarkable report titled "Toward a More Responsible Two-Party System."[1] This Report is today both relevant and disturbing: relevant to our collective past, to current discussions of party reform, and to any serious consideration of political science as policy science and the political scientist as policy scientist; disturbing to any political scientist who believes that the discipline can provide knowledge applicable to the solution of human problems and the achievement of human goals.

The Report was a reform document. Its purpose was to "bring about fuller public appreciation of a basic weakness in the American two-party system." Its style was consistent with its aims. It sought to reach "everyone interested in politics," and so was "written without regard for the customary form in which scholars present their scientific findings." The author asserted that it nevertheless constituted "a summation of professional knowledge" (p. v).

The Report is probably the most publicized document ever published by a committee of the Association. Though the position taken and the arguments presented were by no means new and, in fact, had a long and distinguished history, their statement by the Committee gave them a wide circulation. This was, perhaps, because the statement was brief and nontechnical; perhaps, because it was widely regarded as bearing the imprimatur of the Association, despite a clear statement of the Committee that it spoke only for itself (p. viii).

Hailed as a model for reforming the American political system, denounced as unscientific, slipshod, and mainly mistaken, the Report is a landmark in the history of political science as policy science. Since it aimed at diagnosing ills and prescribing remedies, the Report stands or falls as applied science, as political therapy. To diagnose and treat illness, knowledge is needed. It is not enough to love the patient and abhor disease. The work of the Committee, therefore, stands or falls not simply on its statement of goals but on the quality of its data and theory.

I am chiefly concerned with the Report as policy science, but since its worth as policy science is necessarily related to and dependent on its quality as political science, I propose to present this paper in two parts. Part I will:

(1) summarize the Report briefly
  (a) noting its normative purposes and doctrine and
  (b) describing the Committee's conception of the empirical political world;
(2) examine existing evidence relevant to the Committee's view of political reality; and
(3) speculate briefly about the relation between the Committee's political norms and its view of political reality.

Part II will:

(1) examine the Report as policy science, noting especially
  (a) its failure as policy science,
  (b) the reasons for that failure, and
  (c) some lessons to be learned.

It is well known that much of the best work of empirical political science has been done in the parties field. The result is a literature that is varied, subtle, and rich. In this literature a great deal of consideration has been accorded the major themes of the Committee Report. Most of the points I wish to make in Part I of this paper have been made elsewhere and with fuller relevant documentation. I do not intend to reproduce these discussions in detail, but only to summarize some of the main points and point to sources in which more substantial discussion and documentation can be found.

Part I

The Committee Report: An Analytic Survey

1) The Normative Component. In his study of political parties, Eldersveld commented:

"There has been much theory about party organization, a good deal of it revealing our own normative concerns, but there has been little systematic analysis of the party as an organizational system. The apotheosis of these developments was perhaps reached in 1950 with the edict of the APSA, again after no systematic study of party organization, that our American parties were not democratic, responsible, or effective. . . ."[2]

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* Prepared for and presented at the Sixty-sixth Annual Meeting of the American Political Science Association, Biltmore Hotel, Los Angeles, California, September 8–12, 1970.

[1] "Toward a More Responsible Two-Party System: A Report of the Committee on Political Parties, American Political Science Association," *The American Political Science Review* (Supplement: Vol. 44, September 1950, Number 3, Part 2). Hereafter, both in the text and in the notes, I refer to this document as the Report and, unless otherwise noted, all page numbers cited in the text refer to the Report. This Report can be obtained from Johnson Reprint Corp., 111 5th Ave., New York, New York 10003.

While Eldersveld's judgment may seem harsh, the prescriptive tone and content of the Report are a shock to many contemporary professional readers. The uninhibited exhortation to change may be unexpected from a committee of political scientists setting out to report the American party system. The lack of clarification, justification, or analysis of the norms reinforces its popular, missionary tone. Goals are presented by way of affect laden key concepts or phrases: "*proper* range of choice," "necessity for more *effective formulation* of general policies," "*popular* government," "an *effective* party system," the party system *needed* must be *democratic,* and "*responsible.*"

Together with some unstated assumptions these concepts comprise a normative theory of government contained in the following quotations from the Report:

A) "Popular government in a nation of more than 150 million people requires political parties which provide the electorate with a proper range of choice between the alternatives of action" (p. 15).
B) "The party system that is needed must be democratic, responsible and effective" (p. 17). (After all, who would want a party system undemocratic, irresponsible, and ineffective?)
C) "An effective party system requires, first, that the parties bring forth programs to which they commit themselves . . ." (p. 26) and
D) "That the parties possess sufficient internal cohesion to carry out these programs" (pp. 17–18).
E) "The fundamental requirement of accountability is a two-party system in which the opposition party acts as the critic of the party in power, developing, defining and presenting the policy alternatives that are necessary for a true choice in reaching public decision" (p. 18).
F) "Party responsibility means the responsibility of both parties to the general public, as enforced in elections" (p. 22).
G) "Party responsibility includes also the responsibility of party leaders to the party membership, as enforced in primaries, caucuses, and conventions" (p. 23).

Obviously, this is a theory of democracy which conceives mass participation in policy decisions as crucial (choice among those competing for the right to rule is not enough); which conceives that this participation should be provided by political parties (not through any other agency of the political system); which conceives that only parties can be held responsible (not individuals); which conceives parties (not other institutions in the system) as appropriate agencies for policy making; which conceives the policy process as rational and effective only when it proceeds by way of comprehensive, coherent, long-range planning (not by some variation of disjointed incrementalism).

In sum, the Committee asserted that (1) popular government (democracy) requires party responsibility and does not exist where political parties are not responsible; (2) parties cannot be responsible without cohesion and comprehensive programs; (3) loosely organized parties cannot have either such programs or such cohesion; therefore (4) American parties being neither cohesive, programmatic, or responsible, democracy does not exist in the United States.

Now, the Committee did not draw the conclusion stated in (4), though it is logically derived from the Committee's premises. Instead the Committee delivered warnings such as "This is a very serious matter, for it affects the very heartbeat of American democracy"; and pointed to great dangers lurking around the corner in case of inaction. It described this as an explosive era and asserted: "the political foundation of appropriate governmental programs is very unstable when it is not supplied by responsible party action . . . Should we ever tumble into serious economic crisis for lack of such a firm base, the loss of stature as well as strength may well prove a turning point for freedom throughout the world." It warned that when political parties fail, the people might turn to the President and overextend his power "to a point where it may very well ring in the wrong ending." It predicted that the electorate might turn its back on the parties and noted that continued alienation between voters from "both major parties is an ominous tendency"; finally, it asserted that if something were not done to develop alternative programs "more extreme tendencies to the right or left might be set in motion" (pp. 92–96).

Perhaps the Committee (I should say we) did not fully identify popular government with the availability of a clear-cut electoral choice between alternative policies; perhaps the Committee thought there were degrees of democracy, with the Committee model at one end of the spectrum; perhaps the Committee did not know what it meant. A good case can be made for the latter possibility. There is a great deal of ambiguity in the Report, resulting from a pervasive imprecision of language combined with a total failure to distinguish between normative and empirical statements. Many of the key terms are used both normatively and descriptively, so that it is impossible to be sure at any moment in which terms the Committee was speaking. For example, the Committee asserts,

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[2] Samuel J. Eldersveld, *Political Parties: A Behavioral Analysis* (Chicago: Rand McNally and Co., 1964), p. 14. Eldersveld's study is an extremely important one for the subject matter of the Committee's Report. The tragedy is that it has not been replicated in dozens of other units of government here and abroad.

> "The concern of the parties with candidates, elections, and appointments is misunderstood if it is assumed that parties can afford to bring forth aspirants for office without regard to the views of those so selected. Actually, the party struggle is concerned with the direction of public affairs. Party nominations are no more than a means to this end" (p. 15).

Reading this quotation, it is difficult, if not impossible, to decide whether the Committee is offering an interpretation of reality according to which struggles that appear to deal only with personality or power are actually struggles over policy; or whether the Committee is asserting that the party struggle *should* turn on matters of policy. Take another example: "... we are led nevertheless to conclude that the choices provided by the two-party system are valuable to the American people in proportion to their definition in terms of policy" (p. 15). Is this normative or empirical? Did the Committee mean that choices *should* be valued only in proportion to their definition in terms of policy? This type of normative and empirical ambiguity is characteristic of most of the Report.

Repeatedly the Committee employed stipulative definitions and used them to judge reality. The Committee defined democracy in terms of party government which would substitute partisan mutual adjustment in the party for partisan mutual adjustment in the legislature.[3] It defined rationality in terms of comprehensive planning. By such definitional procedures, preferences were consistently confused with existence and necessity. Ambiguity derives, too, from a continual use of language designed more to stimulate action than thought and from continual efforts to persuade rather than enlighten. Thus, participation, which the Committee regarded as having intrinsic value, is recommended also as a means to other desired ends such as party cohesion, but no evidence is provided to show that the postulated empirical relations actually exist.

Before completing this short summary of the normative aspects of the Report, I should mention also the many pseudo-normative statements in the form of prescriptions and exhortations that, in fact, are empirical statements. These postulate an instrumental relation between a given change or recommended reform and the achievement of the goal of party responsibility. A typical example is the assertion: "In both the Senate and the House, the various leadership groups should be consolidated into one truly effective and responsible leadership committee" (p. 8). The unarticulated term of this proposition, of course, is: "if you want to achieve the type of responsible party government advocated in this Report." Stated in this way the instrumental character of the recommendation is clear. Much of the Report consists of just such prescriptions, and while their plenitude is largely responsible for the normative tone of the Report, they in fact postulate instrumental relations between recommended reforms and stated goals. The test of these statements is empirical: Will the recommended actions lead to the prescribed goals?

In sum, despite some logical and linguistic ambiguities, the Committee postulated a normative democratic theory that was based on a particular reading of British experience. Onto this the Committee grafted selected liberal collectivist, perhaps socialist, assumptions and some traditional American hopes. The existence of two competitive, programmatic, internally democratic parties was central to the conception. The Committee conception of democracy wedded party responsibility to citizen participation within parties. It wedded majority rule to rationalist politics. By making rational choice between alternative coherent programs *the* defining characteristic of popular government and democratic citizenship, the Committee endorsed as normatively valid and empirically feasible a mandate theory of representation and a rational-activist conception of citizenship. Later, I will comment further on the Committee's normative model. Now, however, I wish to examine how the Committee linked its norms with its understanding of politics.

2) The Empirical Component. The empirical component of the Report lends itself more readily to analysis. The Committee held a view about political development, not uncommon at midcentury, that mass electorates and modern technology required programmatic mass membership parties. The continental and British Socialist parties of pre-World War II vintage supplied the model. This expectation concerning political development provided the basis for such assertions as: "... the main trends of American politics have tended to outflank the party system." The Committee indicted American parties not only for being démodé, but for failing to bring about adequate leadership, adequate opposition, and adequate government.

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[3] For an interesting discussion of this and other consequences of the responsible party doctrine, see Charles E. Lindblom, *The Intelligence of Democracy* (New York: The Free Press, 1965), Ch. 20.

Responsibile parties, the Committee argued, would correct these failings. They would provide more effective government and stimulate mass participation. Party government would eliminate the discontent arising from the tendency to legislate through cross-party coalitions. This tendency, the Committee asserted, in one of its many breath-taking feats of mass mind reading, the public regards as "difficult to reconcile with the ballot" (p. 19). Responsible, programmatic parties also would provide better protection against interest groups and would eventuate in more effective opposition. Intraparty democracy in turn would bring more widespread participation in politics, promote internal party unity, and induce new levels of identification with party programs.

No constitutional changes would be required to create responsible, programmatic parties. It would be necessary only to reform institutions controlled by the parties themselves. Naturally, some restructuring of national party organizations would be required to provide strong, representative, policy-oriented leadership. It would be essential to reform the existing relations between state and local parties, on the one hand, and national parties on the other. This could be done as national parties with a strong interest in national issues and national purposes "interested themselves" in Congressional candidates, making sure these candidates were committed to the national programs. It also would be necessary to reform the Congressional committee system, eliminating seniority and providing unified leadership to the two houses. And, of course, it would be necessary to democratize and strengthen the central party machinery; the National Committee would have to be made more representative by apportioning membership on the basis of party votes cast in each state. Further, the National Committee would have to be made subservient to the new and reformed National Conventions, which would also be made more representative and more active, meeting at least every two years. National Policy Councils would be needed to formulate and interpret party platforms. Universal adoption of closed direct primaries would democratize the nominating process. To this package, the Committee added a few extra trifles: abolition of the electoral college, new rules for regulating party finances, the short ballot, four year terms for House members, periodic reapportionment, and the elimination of legal barriers to voting. This then was the recipe offered the American parties and American people for achieving responsible party government and full democracy.

The Committee's version of responsible party government makes policy the central concern of voters, parties, legislators and government. It is the function of parties to develop, through continuous consultation with the rank and file, coherent policy positions that will serve, in Epstein's language, to "structure the electorate" in the next election and thus become the legislative program of the winning party. Parties and candidates become spokesmen for policies and can be held responsible to the extent that they function in this fashion. This model makes a number of assumptions about where decisions can and should be made in the political system. It assumes that

(1) political parties are the agencies that can and should formulate such policies;
(2) the policies formulated can be so coherent they provide a single focus of attention for the voter (such focus is a clear requirement of the mandate theory of representation that lies at the heart of responsible party doctrine);
(3) broad, coherent, comprehensive planning will result in better decisions (better for whom is not spelled out) than will some form of disjointed incrementalism;
(4) parties are an independent variable in the political system, and by changing parties it will be possible basically to alter the system so as to eliminate the separation of powers and federalism, to provide unified and centralized government.

The Report was neither universally acclaimed by the American people, who—as might have been predicted—ignored it, nor by politicians, most of whom also ignored it, nor by students of political parties, who have given it a great, perhaps inordinate, amount of attention. In the two decades since its publication it has more often served as a foil than as a model, and the one party leader who tried to implement its proposals failed almost completely.[4] A number of the major studies of American parties that have appeared since the Report include explicit criticisms of it.[5] Collectively, these criticisms amount to an indictment of the Report's analysis and recommendations as ill-conceived, ill-formulated, and, most often, simply mistaken about parties, about people and about political systems.

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[4] See Daniel M. Ogden, Jr., "Party Theory and Political Reality Inside the Democratic Party" (Paper prepared for the 1960 Annual Meeting of the APSA in New York City. The paper may be obtained from University Microfilms, Ann Arbor, Michigan.)

[5] Some examples are: Austin Ranney and Willmoore Kendall, *Democracy and the Party System* (New York: Harcourt, Brace and Co., 1956), Ch. 22; William Goodman, *The Two Party System in the United States*

The Committee's scope was broad, the literature relevant to it is voluminous. I propose to comment only on those aspects of literature most relevant to key arguments in the Report. I will discuss four questions; each involves assumptions and assertions about the nature of the political world that seem to me crucial to Committee doctrine. The questions are:

(1) Can political parties formulate policy, and should they?

(2) Can voters discriminate among policies, and will they?

(3) Is it like that in Britain?

(4) Are parties the keystone of the political arch?

1) Can Political Parties Formulate Policy, and Should They? No aspect of Committee doctrine is clearer than the assumption that parties can and should formulate policies for the government of the nation. Yet, as various subsequent writers have pointed out, it is by no means clear that political parties are competent, efficient, or appropriate institutions for the formation of policy which is to be binding upon legislators and executive leaders. Epstein has commented: ". . . parties, given their electoral function, are not especially well qualified to assume additional functions of policy making. Issues would have to be unusually simple for parties to qualify. Or there would have to be many more than two parties—enough, in fact, to provide specific policies for each substantial set of preferences within the electorate."[6] In a recent article Anthony King asserts that there has been a "general decline of parties as bearers of ideology and a concomitant decline everywhere in their role in policy formation."[7] The Committee probably conceived parties as formulating only broad policies on major issues, but it is nonetheless difficult to conceive issues on which parties could formulate relevant policies and it is still more difficult to conceive issues on which policy could be developed through rank and file discussion at the grass roots level.[8] It is not enough that the issues be broad ones. Solving broad problems requires breaking them into parts, and solution to parts almost always requires technical expertise. Big issues—what to do in the Middle East, how to handle military manpower requirements, crime control, urban redevelopment—cannot be intelligently or responsibly dealt with in the abstract. Dealing with them requires a mass of contextual data that specify, qualify, and pro-

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(Princeton, N.J.: D. Van Nostrand and Co., 1964), Ch. 25; Allan P. Sindler, *Political Parties in the United States* (New York: St. Martin's Press, 1966), Ch. 5; Frank J. Sorauf, *Political Parties in the American System* (Boston: Little, Brown and Co., 1964), Ch. 9; Ivan Hinderaker, *Party Politics* (New York: Holt, 1956); Judson L. James, *American Political Parties: Potential and Performance* (New York: Pegasus, 1969), Ch. 1, pp. 26–28; James makes an interesting distinction between "party government" which he criticizes and "responsible parties" which he approves, pp. 28–32; Leon Epstein, *Political Parties in Western Democracies* (New York: Praeger, 1967). Epstein's book is particularly valuable. He discusses most of the issues raised by the Report and provides comparative data, primarily U.S. and Britain but including a number of other countries. There are, of course, numerous other critics. I know of no major work that undertakes a defense of the Report but there are distinguished political scientists who support responsible party government and argue the case much more cogently than the Committee. These include E. E. Schattschneider, *Party Government* (New York: Farrar and Rinehart, 1942), *The Struggle for Party Government* (College Park, Md.: The University of Maryland Press, 1948), James McGregor Burns, *Congress on Trial* (New York: Harper and Brothers, 1949), *The Deadlock of Democracy* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1963), and Stephen K. Bailey, *The Condition of our National Political Parties* (New York: The Fund for the Republic, 1959). It is interesting that Marvin Surkin and Alan Wolfe ("The Political Dimension of American Political Science," *Acta Politica*, 5 [October, 1969, p. 47]) describe the Committee Report as a turning point in American political science, presumably marking the transformation of political science from a "reformist" to a "conservative" orientation. They do not argue the case, however. For some excellent collections of articles that bear on various aspects of the Report, see David W. Abbott and Edward T. Rogowsky, *Political Parties, Leadership, Organization, Linkage* (Chicago: Rand McNally & Co., 1971); Edward C. Dreyer and Walter A. Rosenbaum, *Political Opinion and Behavior: Essays and Studies* (Belmont, Calif.: Wadsworth Publishing Co., 1970); Marian D. Irish, Robert L. Lineberry and James W. Prothro, *Readings on the Politics of American Democracy* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1969); William J. Crotty, Donald M. Freeman, and Douglas S. Gatlin, *Political Parties and Political Behavior* (Boston: Allyn and Bacon, Inc., 1966); William J. Crotty, ed., *Approaches to the Study of Party Organization* (Boston: Allyn and Bacon, Inc., 1968); Donald G. Herzberg and Gerald M. Pomper, *American Party Politics: Essays and Readings* (New York: Holt, Rinehart and Winston, 1966); William E. Wright, ed., *Comparative Study of Party Organization* (Columbus, Ohio: Charles E. Merrill Publishing Co., 1971).

[6] Epstein, *op. cit.*, p. 269.

[7] Anthony King, "Political Parties in Western Democracies," *Polity*, 2 (Winter, 1969), pp. 111–141, esp. 135–137.

[8] Reading Eldersveld's *Political Parties* is indispensable to judgment on this and a variety of other matters discussed in the Report. See, for example, his discussion of the ideological structure of the party, Ch. 8; of ideology and electoral behavior, Ch. 19; of the party as a decisional system, Ch. 15.

ject. Only scholastic debates feature the big issues broken into one pro and one con, and it is significant that in these debates the prize goes to the best argument, not to the best solution. It is not necessary to believe that we have arrived at the "end of ideology" to know that most problems of public policy have high requirements for diverse types of technical knowledge, and, perhaps more important, that most "complex decision making is not synoptic but is fragmented, disjointed, and incremental."[9] Policy making almost always, if not always, begins from where we are and moves by modifications of what exists. Policy makers can rarely write on a clean slate. Policy making requires knowledge of existential situations and of the consequences of alternative policies. Political parties do not have the batteries of experts needed for such policy functions, nor even the staffs required to elicit the opinions of experts and weld them into a coherent program. If policy and program "trickle down" from above there will be no mass participation. If policy and program "bubble up" from below, there will be little use of expert knowledge. To meet the Committee's requirements, programs and policies would have to be arrived at through continuous consultation of both the experts and the rank and file. Policies would emerge from a complex process of aggregating interests in the context of relevant expertise and, like all such collective efforts, would embody many compromises. Further, there is no certainty that either the "trickle down" or the "bubble up" method or any combination of the two would produce programs sufficiently integrated to satisfy the Committee's requirements for coherence.

If the programs and policies developed did not differentiate the parties from one another, they would not give the voters the clear choice the Committee felt essential. Yet, in a nation that has been famous—at least until recently—for ideological unity, there is little evidence that the recommended institutional practices would leave the parties very far apart. For the parties to arrive at reliably different policy positions on broad issues, they would have to start from different premises, be guided by different values, employ different cognitive modes, or seek different goals. In other words, they would have to embrace different comprehensive ideologies. They would have to differ on goals, not just priorities or means. The French Communist Party and the Gaullists, for example, devise different prescriptions for almost all important political problems. They do so because they have different goals, because they make distinctive assumptions about the nature and destiny of France, about how society is and ought to be organized, about the obligations of individuals and classes, and, above all, about the organization of political power. But in America in 1950 there were no such divisions about goals, about the purposes of authority. The Committee maintained that programmatic parties did not require ideological divisions in the electorate; and acknowledging the absence of "real ideological division in the American electorate," the Committee also asserted that differing "programs of action presented by responsible parties for the voters' support could hardly be expected to reflect or strive toward such division" (pp. 20–21).

Most students of parties think otherwise. It is a commonplace in the literature of comparative political institutions that programmatic parties exist only in times and places characterized by significant ideological cleavages. Epstein, for example, comments: "Conflicting value systems, each capable of enlisting considerable support in a given society, are thus essential to the existence of major programmatic parties."[10] Dahl writes: "Obviously, unless attitudes are highly polarized it is impossible to divide a population into two like-minded collections of people. No matter what criterion is used for dividing people, within each of any two categories there will be conflicting views."[11] The point seems clear enough. Coherence on a range of different issues is provided by a unifying perspective on the past, present, and future. Where identifications and expectations are shared, demands are likely to be shared also. Lack of concern with the social and cultural environment is one of the most striking aspects of the Report. Nowhere is this more evident than in the Committee's unconcern with the social, cultural, or even ideological bases of its proposed party system. Perhaps the Committee did not think the environment of the system important. Certainly the Report reflects no concern for the relations between social, cultural, and political cleavages. Perhaps the Committee believed in the autonomy of the political system; that would have been consistent with its optimism about the chances for comprehensive and purposeful political reform. But, even if the parties found a way to make policy, American and British experience demonstrate how difficult it is to get such policy accepted by candidates and legislators.[12]

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[9] Lindblom, *op. cit.*, p. 331. For the argument, see Chs. 9 and 10. Lindblom argues that the nature of complex decision making is such that "multiplicity of independent decision makers is itself a source of rationality" as is the form of their interaction. See also Otto A. Davis, M. A. H. Dempster and Aaron Wildavsky, "A Theory of the Budgetary Process," *American Political Science Review*, 60 (September, 1966), pp. 529–547.

[10] Epstein, *op. cit.*, p. 262.

[11] Robert A. Dahl, *Pluralist Democracy in the United States* (Chicago: Rand McNally and Co., 1967), p. 370. Dahl's book is relevant to most of the matters raised in the Committee Report. Every practicing politician knows the truth of the proposition that "policy is the mother of dissension."

2) Can Voters Discriminate among Policies, and will they? The Committee made some extraordinary assumptions about the role and nature of rationality in the political process. Its model of the party system stresses cognitive elements to the exclusion of all others. Ideas are considered the *raison d'être* of political parties, the motivating force for participation, the determinant of voting behavior. Parties come into existence to support a distinctive conception of what governmental policy ought to be. Ideas—not interests, habit, and identifications—provide the motor power, attraction, and goals. Candidates stand for ideas and represent ideas. Personality and character are basically irrelevant. All that matters to a candidate's political role is his relation to public policy. Therefore, the voting decision is conceived as an exercise in ratiocination. The voters listen to the ideas and the programs of the parties. The voters then consider, compare, and choose. The Committee version of political life is the rational-activist model *par excellence*. In fact, it may be the purest statement of a purely cognitive conception of democratic politics in modern political literature.

This conception, described by Schumpeter as the 18th-century classical model and vigorously criticized by him and by most contemporary students of politics, is consistent neither with the findings of modern empirical research, nor with the views of most political philosophers.[13] Most of the leading thinkers of the "great tradition" of political science assigned a greater importance to nonrational factors than does the rational-activist Committee model. Plato's emphasis on the reciprocal influence of character, socialization, economic factors and goals demonstrates his awareness of the complexity of the sources of political behavior. Aristotle's discussion of the influence of the distribution of wealth and occupations confirms his understanding of the importance of the nonrational factors to the functioning of the polity. Montesquieu's elaborate and suggestive discussion of the socioeconomic correlates of the political system speaks for his conception of the relation of nature, nurture and constitutions. The *Constitution of Poland* and *Émile* are Rousseau's tribute to the nonrational foundations of political behavior. Hobbes, Hume, Burke, Bagehot and Madison differed on many aspects of politics but agreed in assigning a large importance to nonrational factors.[14]

The emphasis of the Report on cognition is both descriptive and normative. A close reading makes clear that the Committee believed *both* that people *are* linked to politics chiefly through policies and that this *ought* to be the case. The chief failure of parties, therefore, is their failure to provide the electorate the rational choice for which the electorate yearns and which alone can render politics meaningful to them. The nonrational bases of political behavior are either disapproved and dismissed or ignored. An interest is not conceived as deriving from social identity, as naturally and legitimately influencing political action, but as an extrinsic, undesirable, and probably immoral interference with the public interest, which is nowhere defined. Voting is not a chance for self-protection, or to throw the rascals out, or to say where the shoe pinches, but a chance to choose between rational policy alternatives.

The Committee's premises are those of 19th century English Radicalism, garnished with touches of Rousseau. There is no place in the Committee model for habit, that enemy of ratiocination (or friend, considering the time it frees for thought), for authority, expertise, expression of group interests. The electorate was conceived as consisting of discrete, thinking individuals, not persons bound into webs of identifications—ethnic, religious, economic, sectional, political—that condition individual political behavior.[15]

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[12] Epstein, *op. cit.*, Ch. 11.

[13] See Joseph Schumpeter, *Capitalism, Socialism and Democracy* (New York: Harper and Brothers, 1942), Part IV. Other critics include Henry Mayo, *An Introduction to Democratic Theory* (New York: Oxford University Press, 1960); James Q. Wilson, *The Amateur Democrat: Club Politics in Three Cities* (Chicago: The University of Chicago Press, 1962); Seymour M. Lipset, *Political Man* (Garden City, N.Y.: Doubleday, 1960); Bernard Berelson et al., *Voting* (Chicago: The University of Chicago Press, 1954), Ch. 14; Robert A. Dahl, *A Preface to Democratic Theory* (Chicago: The University of Chicago Press, 1956), *Who Governs?* (New Haven: The Yale University Press, 1961); V. O. Key, *Public Opinion and American Democracy* (New York: Alfred A. Knopf, 1961); and Gabriel Almond and Sidney Verba, *The Civic Culture* (Princeton: The Princeton University Press, 1963). In fact, most contemporary students of political parties and voting behavior have been critical of the APSA Committee's model.

[14] The critics of the rational activist model of the Committee have their critics; see Lane Davis "The Cost of the New Realism," *Western Political Quarterly*, 17 (1964), pp. 37–46 and Jack L. Walker's "A Critique of the Elitist Theory of Democracy," *American Political Science Review*, 60 (June, 1966), pp. 285–295. Both of these articles are reprinted in Henry Kariel's *Frontiers of Democratic Theory* (New York: Random House, 1970), pp. 213–247. Kariel's book is the best collection of materials setting forth both points of view in this controversy, mainly a difference between descriptive and normative viewpoints.

The cumulative impact of voting studies on the Committee model of the responsible party doctrine is, quite simply, devastating. Even if two programmatic but nondivisive parties were conceivable in the United States, the likelihood that their programs could be communicated to a mass electorate which would then choose in terms of the two alternatives structured by the parties, is inconceivable. Voting research confirmed what earlier work had frequently hypothesized: political behavior, like human behavior generally, is rooted in the whole person, through identifications internalized early in life and retained with tenacity. Continuing replications of voting studies demonstrated that man is, to be sure, purposive, but that his purposes derive to a large extent from the web of identifications and expectations that anchor and orient him in society. The significance of ethnic, religious, and racial identifications (always well understood, however, by political leaders who looked for Goldbergs and O'Connors, Bolzanos and Ciccolinis) has been established in a long series of electoral studies running from Lazarsfeld's efforts in Elmira and in Erie County to the Survey Research Center's most recent work.[16]

Other studies established the political functions of occupational peers in the electoral decision, and the importance of that other small group, the family, has been confirmed in study after study. Socialization studies reenforced voting studies, finding not only that family is a breeding ground of values and orientations generally relevant to politics but also that family links individuals to the political world by transmitting party identification and, as two decades of research on parties and voting behavior have demonstrated, "few factors are of greater importance for our national elections than the lasting attachment of tens of millions of Americans to one of the parties. These loyalties establish a basic division of electoral strength within which the competition of particular campaigns takes place."[17]

The implications of party identification for the responsible party doctrine are of capital importance. As Allan Sindler has said: "The real world of party identifiers contrasts starkly with the picture of the 'democratic citizen' so pervasive in our civics texts and political rhetoric."[18] Party identification is a stable, enduring, nonrational aspect of the individual's orientation to the political world. The party identifier approaches any given electoral contest with a predisposition toward a given party that antedates the differences between the parties in that election. To be sure, party identification predisposes, it does not determine. But, at least in stable political systems of the Anglo-Saxon type, it is a powerful predisposition.[19] It is clear that all the various group identifications that antedate any given electoral contest constitute nonrational factors which influence the focus of voter attention, the reception of campaign stimuli, and the ultimate voting decision itself.

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[15] The failure of the Committee to take note of the group theories of politics is significant and shows to what extent the Report constituted advocacy without even a token attempt to present both sides. As noted elsewhere, Pendleton Herring's *The Politics of Democracy* (New York: W. W. Norton and Co., 1940) had appeared ten years before the Committee Report and had been well received. The work of Arthur Bentley, *The Process of Government* (Bloomington, Ind.: The Principia Press, 1949) was certainly well known to most members of the Committee. Graham Wallas published his *Human Nature and Politics* in the same year as the first edition of Bentley (1908) and Harold Lasswell's *Psychopathology and Politics* was published in 1930 (Chicago: The University of Chicago Press, 1930). Further, Sigmund Freud, Walter Bagehot, Charles Merriam, and others had made clear the importance of the nonrational in behavior, including political behavior.

[16] Paul F. Lazarsfeld, Bernard R. Berelson and Hazel Gaudet, *The People's Choice: How the Voter Makes up His Mind in a Presidential Campaign* (New York: Duell, Sloan, and Pearce, 1944); Bernard R. Berelson, Paul F. Lazarsfeld, and William N. McPhee, *Voting* (Chicago: University of Chicago Press, 1954); Angus Campbell, G. Gurin, and Warren Miller, *The Voter Decides* (Evanston: Row, Peterson, 1954); Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald Stokes, *The American Voter* (New York: John Wiley and Sons, Inc., 1960). For a brief summary of knowledge gained from survey research, see the excellent brief books by William H. Flanigan, *Political Behavior of the American Electorate* (Boston: Allyn Bacon, Inc., 1968) and Fred I. Greenstein, *The American Party System and the American People* (Englewood Cliffs, N.J.: Prentice Hall, Inc., 1963) which summarize a considerable amount of survey and other relevant data. Also, see Raymond E. Wolfinger, "The Development and Persistence of Ethnic Voting," *American Political Science Review*, 59 (December, 1965), pp. 896–908.

[17] Campbell, Converse, Miller and Stokes, *op. cit.*, p. 121.

[18] Sindler, *Political Parties in the United States* (New York: St. Martin's Press, 1966), p. 90.

[19] In some democratic societies, notably nations noted for institutional fragility, party identifications may be less stable than ideological orientations. See Philip E. Converse and George Dupeux, "Politicization of the Electorate in France and in the United States" reprinted in Angus Campbell, Gerald Gurin, and Warren Miller, *Elections and the Political Order* (New York: John Wiley and Sons, 1966), pp. 269–291. Also see Jeane J. Kirkpatrick, *Leader and Vanguard in Mass Society: A Study of Peronist Argentina* (Cambridge, Mass.: The M.I.T. Press, 1971).

These social and political identifications are, of course, only one source of nonrational electoral behavior. Low information levels are another bar to rational decision of the type recommended by the Committee model. The evidence on the information levels of mass electorates is overwhelming. *It is significant that neither side in the "responsible electorate" debate attributes to the electorate the information level about specific issues and policies required by the Committee model.*[20]

There is, of course, a great deal we do not know about the coherence, rationality, and consistency of issue orientations, about how issues cluster and hang together and acquire salience, but we have learned certain basic—for some traumatic—facts about voter information levels. We know, for example, that even in such an apparently single-issue campaign as the New Hampshire Democratic presidential primary in 1968, among McCarthy supporters "those who were unhappy with the Johnson administration for not pursuing a harder line against Hanoi outnumbered those advocating a withdrawal from Vietnam by nearly a three to two margin."[21] And we know, despite some argument to the contrary, that low voter information levels are not characteristic just of the American system,[22] or just of one socioeconomic group. If Americans alone were badly informed, it might be reasonable to blame the American system. If the poor alone were ignorant and pathetic, it might be reasonable to ask about the poor: "Are people apathetic because they feel their political efforts will be futile?"[23] But data accumulated on other countries and other classes demonstrate that for most people at all educational and economic levels, political issues have little salience, and perhaps in consequence, voter information levels are low. In any event, even a sympathetic reading of the Committee Report demonstrates that the Committee model of electoral choice requires a higher level of voter information than exists in any known mass electorate.

At the heart of the Committee's model are distinctive party platforms, revised biennially, emphasizing "general party principles and national issues, . . ." which would constitute a "binding commitment on all candidates and office holders of the party, national, state and local" (p. 56). Thus, in addition to a high level of voter information, the Committee's model requires a single point of focus for the attention of the electorate. Like all mandate theories of representation, the responsible parties doctrine conceives that (1) all the electorate perceives (or ought to perceive) the same issues, and (2) the same issues are the determinant of voting for all voters. Voting studies, on the other hand, have demonstrated that even in such highly ideological movements as the Wallace party in 1968, partisans differ in their perceptions of the movement's *raison d'être.*

Another quite different finding of voting research has significant implications for the Report. At the same time that researchers discovered (confirmed) widespread ignorance about specific political issues, they also found significant differences in the parties' images. Most voters perceived the parties as having different group bases, different clienteles, and different competences.[24] Still other researchers argued that these perceived differences were grounded in reality; that, in fact, the objective predictable differences between the parties were greater than the Committee believed.[25] A series of studies have confirmed that leaders of the two parties have distinctive, though never monolithic views about policy questions,[26] that the legislative records of the two parties are quite different,[27] and that "party affiliation of legislators tends to be the best single predictor of their roll call votes."[28] Thus there is considerable evidence that American parties have distinctive leadership, distinctive legislative behavior, distinctive clienteles and distinctive images. To the extent this is true, it can be argued that American parties provide the electorate all the choice a mass electorate will perceive, and all the coherence desired by a heterogeneous electorate with multiple foci of attention.

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[20] Those who argue the case that voters are more "rational," and that issues have greater salience than attributed to them by early SRC studies such as *The American Voter* and especially by Philip E. Converse, "The Nature of Belief Systems in Mass Publics" in *Ideology and Discontent,* ed. David E. Apter (Glencoe: The Free Press, 1964), pp. 238–245, define "issue" not in programmatic terms but as broad orientations toward salient aspects of social and political life, e.g., David E. RePass, "Issue Salience and Party Choice," *American Political Science Review,* 65 (June, 1971), pp. 389–400 and David M. Kovenock, Philip L. Beardsley and James W. Prothro, "Status, Party, Ideology, Issues and Candidate Choice: A Preliminary, Theory-Relevant Analysis of the 1968 American Presidential Election" (mimeographed paper prepared for the 8th World Congress of the International Political Science Association, Munich, Germany, Aug. 31-Sept. 5, 1970). For an excellent discussion of the ambiguity of the concept of "political issue," see Butler and Stokes, *op. cit.,* pp. 174–175. Samuel Huntington has an interesting article, written in 1950, making a statistical analysis of ". . . quantitative and qualitative differences" between the two major parties. He found that "contrary to the traditional thesis, the quantitative differences between the parties tend to be inversely proportional to the qualitative differences between the parties;" "A Revised Theory of American Party Politics," *American Political Science Review,* 44 (September, 1950), pp. 669–677.

[21] Philip E. Converse, Warren E. Miller, Jerrold G. Rusk, and Arthur C. Wolfe, "Continuity and Change in American Politics: Parties and Issues in the 1968 Election," *American Political Science Review,* 63 (December, 1969), p. 1092.

[22] To the contrary, Almond and Verba, *op. cit.,* p. 174, present evidence that Americans are among the most well informed of the five nations surveyed.

[23] Lewis Lipsitz, "Forgotten Roots" in Kariel, *op. cit.,* p. 403. This article argues the position, among others, that low information levels of the American poor constitute a failure of the system to "make democracy more meaningful" and that other interpretations constitute a failure of political scientists.

[24] See Campbell, Converse, Miller and Stokes, *op. cit.;* Campbell, Gurin and Miller, *op. cit.* For a valuable discussion of the problem in Britain, see David Butler and Donald Stokes, *Political Change in Britain: Forces Shaping Electoral Choice* (New York: St. Martin's Press, 1969), Ch. 16 "Images of the Parties."

3) Is it like that in Britain? It has been generally believed that the APSA Committee, like a number of earlier advocates of the responsible party doctrine (Wilson, for example), saw the British parties and party system as empirical referents for its model and was, in essence, recommending that American parties become like British parties; more accurately, like British parties were believed to be.[29] This interpretation of British parties and the British system may have been dominant in America in 1950. On the other hand, Pendleton Herring's voice had been raised against this view as early as 1940,[30] and the Committee position was not shared by leading interpreters of the British constitution. Ivor Jennings, for example, had explicitly attacked the mandate theory as "political cant" useful as myth, nonsense as description.[31] Not long after the Committee Report appeared, David Butler (see note 30) published "American Myths About British Parties," in which he fired a barrage of data and arguments demonstrating that British parties did not conform to the responsible parties model. Butler, like Jennings before him, asserted that British parties generally did not offer the electorate the clearly differentiated choice that many supposed. And he reminded readers of what Schattschneider and others had emphasized, that British parties were not characterized by intraparty democracy. A substantial body of subsequent research supplemented and reenforced this view of British parties. Research on British politics, especially the Nuffield studies, has revealed that large numbers of Englishmen fail to distinguish clear programmatic differences between the Conservative and Labour parties. Butler and King, for example, report that the percentage perceiving little difference between the Conservative and Labour parties rose from 34 per cent in 1959 to 49 per cent in 1964.[32] At the mass level, the perception of British parties turns out to be very similar to the perception of American parties by the American electorate.

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[25] A most effective argument—based on survey data—that voters perceive distinctive differences between the parties and make a rational choice in V. O. Key, Jr. (with the assistance of Milton Cummings, Jr.), *The Responsible Electorate: Rationality in Presidential Voting 1936–1960* (Cambridge: The Harvard University Press, 1966). For further emphasis on voters' rational guides to action see Arthur S. Goldberg "Social Determinism and Rationality as Bases of Party Identification" *American Political Science Review,* 63 (March, 1969), pp. 5–25. For a recent examination of the relation between issue saliency, party choice and voting behavior, see RePass, *op. cit.,* and Kovenock, Beardsley and Prothro, *op. cit.*

[26] Herbert McClosky, Paul J. Hoffman and Rosemary O'Hara, "Issue Conflict and Consensus Among Party Leaders and Followers," *American Political Science Review,* 54 (June, 1960), pp. 406–427; Eldersveld, *op. cit.,* Ch. 8.

[27] Julius Turner, *Party and Constituency Pressure on Congress* (Baltimore: The Johns Hopkins Press, 1951); Duane Lockard, *New England State Politics* (Princeton: Princeton University Press, 1959); and Congressional Quarterly Service, *Congress and the Nation,* 1945–1964.

[28] William J. Crotty, Donald M. Freeman and Douglas S. Gatlin, *Political Parties and Political Behavior* (Boston: Allyn and Bacon, Inc., 1966), p. 453. Also, see Duncan MacRae, *Dimensions of Congressional Voting* (Berkeley; University of California Press, 1958), David B. Truman, *The Congressional Party* (New York: John Wiley and Sons, 1959) and Lewis A. Froman, Jr. "Inter-party Constituency Differences and Congressional Voting Behavior," *American Political Science Review,* 57 (March, 1963), pp. 57–61. Miller and Stokes, while finding that constituency influences voting, comment that: ". . . no single tradition of representation fully accords with the realities of American legislative politics. The American System is a mixture, to which the Burkean, instructed delegate, and responsible party models all can be said to have contributed elements." Warren E. Miller and Donald E. Stokes, "Constituency Influence in Congress," *American Political Science Review,* 57 (March, 1963), pp. 45–56.

[29] Having been a member of the Committee, I know that the British model was significant for a number of its members. The Committee emphasized that many "Americans have argued that something like the British system of responsible cabinet government would have to be grafted on to ours before an effective party system could come about in the United States." The Report then noted that such a system made for responsibility and that parties "play a key role in it." While rejecting outright adoption of the cabinet system by constitutional amendment, the Committee made clear that it thought most of the benefits could be gained by adaptation within the constitution and said that "It is logical *first* to find out what can be done under present conditions to invigorate the parties before accepting the conclusion that action has to begin with changing a constitutional system that did not contemplate the growing need for party responsibility when it was set up" (pp. 35–36, emphasis mine). Certainly the responsible parties system was widely considered to be the British system. This view was widespread enough for the *Virginia Quarterly Review* to publish David Butler's article, "American Myths about British Parties," attacking the view that the British parties fitted the responsible parties model (*Virginia Quarterly Review,* 31 [Winter, 1955], pp. 45–56).

[30] *The Politics of Democracy* (W. W. Norton and Co., 1940), pp. 107–116 passim.

[31] William Ivor Jennings, *Cabinet Government* (New York and Cambridge: The Macmillan Co., 1936), pp. 389–390.

The APSA Committee's conception of the British model was almost surely derived from the apparently sharp programmatic differences in the first post-World War II election, certainly not from the preceding decade of National Governments. No subsequent British election has even appeared to pose a choice between coherent, comprehensive, and differentiated programs. Instead successive elections have featured marginal differences in program. Recent experience, including Heath's victory in 1970, raises a question about whether even perceived differences may not derive as much from public relations as from actual programs. Gilbert and Sullivan were probably more right about British politics than were the advocates of party government. Most Englishmen, like most Americans, are led by stable hereditary party identification to support a particular party. While the irrational aspects of party identification have probably been overdrawn (since a whole policy-impregnated set of orientations are inherited simultaneously with party identifications), there is a significant difference between an attachment deriving from associations and identifications and an attachment deriving from pure ratiocination. It is a difference crucial to the Committee model.

Another blow to the rational-activist theory of British political behavior has come through increased sensitivity to and understanding of interest groups in the British system. One of the great concerns of the Committee was to have party overcome interest groups; the Committee probably shared to some extent the then widespread illusion that interest groups did not exist in Britain. But the day is now long since gone when professors of British politics could (and did) assert that there were no lobbies in Britain. Instead we are increasingly aware of how British party programs reflect aggregated group interests, and how group identifications relate voters to parties.[33]

The version of British political experience encapsulated in the responsible parties model is one from which all nonrational, nonindividualist elements of the British constitution have been stripped. The behavioral accretions of centuries, which usage rather than reason allied to democratic practices—the deference and the constitutional fictions celebrated by Bagehot, damned by Dicey, and described by Jennings—are ignored in the responsible parties model. But their importance has been confirmed by all recent studies of political culture.[34]

As for intraparty democracy, even at the time of the Report, most students of British parties realized that the Parliamentary parties dominated the policy making process. Pollock pointed out in 1930 that in regard to party organization:

> "In theory English party organizations are quite democratic and anyone can fill even the highest office. But in practice the higher positions are restricted. Through the maze of Committees the discerning eye can see that the leader of the party holds the power and that the party is expected to follow his lead. The organization is his tool and responds to his decision."[35]

In postwar conflicts between the Party Conference and the Parliamentary Labour Party, the PLP, without ignoring Conference policy, had nonetheless refused to surrender to it. In fact, the elected office holders in any democratic system are likely to place their responsibility to the electorate ahead of their responsibility to party membership. The experience of American parties with party or policy councils set up outside Congress is testimony to the difficulties.[36] Epstein concludes that only the assumption that a working-class party represents a majority of the population could lend credibility to an organized membership's claim to policy-making authority. "Otherwise its credentials seem less legitimate than those of public office holders whose policies are tailored to the electorate. The membership must itself be conceived as representing the majority of the population before it can be regarded as any more than another interest group seeking to fashion policy."[37]

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[32] David E. Butler and Anthony King, *The British General Election of 1964* (London: The Macmillan Co., 1965), p. 155. Also see David Butler, *The British General Election of 1951* (London: The Macmillan Co., 1952), *The British General Election of 1955* (London: Macmillan Co., 1955); David Butler and Richard Rose, *The British General Election of 1959* (London: The Macmillan Co., 1960). For an excellent recent discussion, Butler and Stokes, *op. cit.*, especially Ch. 8, but also Chs. 9, 10, 15, 16.

[33] See especially Samuel Beer, *British Politics in the Collectivist Age* (New York: Alfred A. Knopf, 1965). Also see Butler and Stokes, *op. cit.*, Chs. 4, 5, 6, 7.

[34] Notable among these is Almond and Verba, *op. cit.*, which also explicitly relates its findings to the rational-activist model.

[35] James K. Pollock, "British Party Organization," *Political Science Quarterly* (June, 1930), p. 163, quoted in Herring, *op. cit.*, p. 213.

[36] For a valuable discussion of this point, see Epstein, *op. cit.*, Ch. IX, "Program Policy and Organized Membership." Epstein's book is the most valuable single reference for the problems discussed in Part I of this paper.

At the same time that events and analyses were making clear the dominance of parliamentary parties in policy making, other studies revealed the unsuspected power of constituency parties in candidate selection. Ranney, Epstein and others have confirmed that, despite appearances to the contrary, central offices of both the Labour and Conservative parties play a subordinate role in candidate selection.[38]

These findings in turn have led to a re-evaluation of the sources of cohesion of British parties. It appears that cohesion is related more to the operation of the parliamentary system in the British context than to such intraparty factors as central control of candidate selection, membership control over policy formation, or the existence of coherent, comprehensive programs.[39] If cohesion derives from the dynamics of the parliamentary system in the context of British political culture, rather than from the structure of parties, the British model could not provide much hope that changes in U.S. party structure would produce comparable cohesion here. Austin Ranney argued that the Committee should, if it wanted party government, work for "acceptance of the whole package of majority rule democracy."[40] But apparently it was beliefs about the nature of the political world that prevented the Committee from opting for constitutional reform.

4) Are Parties the Keystone of the Political Arch? The APSA Committee made two basic assumptions about constitutional engineering:

A) that it would be more practicable to attempt to change the party system than to amend the constitution (pp. 35–36);

B) that by changing the parties and party system the basic nature of the whole system could be changed (pp. 35–36).

These assumptions are not self-evident, nor are they held by a very large number of political scientists. There was and is a good deal of evidence that it is often easier to change constitutional arrangements than to restructure the expectations and behavior of great numbers of participants. In dismissing constitutional reform, the Committee assumed the primacy of politics in determining the character of the political system (a perfectly respectable position taken by Plato, Machiavelli, Mosca and others) and the primacy of political parties in politics. In arguing that party was the key to constitutional reform, the Committee assumed that parties were an independent variable in democratic political systems; that the character of the system depended on the character of the parties, not vice versa. This view may not be ultimately mistaken, but it has been questioned by succeeding generations of students of parties. Certainly, if Eldersveld's theory of parties is correct (and it has much to recommend it), the character of the party system in a democracy is substantially fixed by the democratic character of the system.[41] In a recent article Anthony King deplored the tendency of political scientists to overestimate the importance of parties and to exaggerate their influence within political systems.[42] Epstein, too, has questioned why parties "should be thought so important that their effectiveness determines the effectiveness of the system."[43] And Allan Sindler asserts: "The prospects for an altered party system are thus more dependent upon contextual and environmental changes than the CPP (Committee on Political Parties) is prepared to concede."[44]

Most contemporary students of comparative politics, skeptical of single-factor theories of causation, emphasize the interrelation of political culture, institutions, and constitutional order, not merely on the grounds that everything is related to everything, but rather that parties are a dependent variable, institutions whose form and function largely depend on both the society and the constitutional system. This view does not imply that parties are unimportant, but rather that they are less important than once believed.[45] In recent years, many students of political parties have asked, with Leiserson, "Are particular sorts of party ideologies *necessarily* associated with types of political structure, such as one party, two party, or multi party systems? Are they an independent force, an autonomous factor in the molding of political systems?"[46]

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[37] Epstein, *op. cit.*, p. 314.

[38] Austin Ranney, *Pathways to Parliament* (Madison: The University of Wisconsin Press, 1965). This is the most exhaustive study of the nominating process in the U.K. Also see his "Central Guidance of Parliamentary Candidate Selection in Britain" paper delivered at the 6th IPSA Congress in Geneva, 1966; and Epstein, *op. cit.*, Ch. VIII, "Candidate Selection."

[39] Leon Epstein, "Cohesion of British Parliamentary Parties," *American Political Science Review*, 50 (June, 1956), pp. 360–377; and *British Politics in the Suez Crisis* (Urbana: The University of Illinois Press, 1964), Chs. 5 and 6. An excellent brief discussion of the problem is to be found in his *Political Parties in Western Democracies*, Ch. XII "The Governing Function."

[40] "Toward a More Responsible Two-Party System: A Commentary" *American Political Science Review*, 45 (June, 1951), pp. 488–499.

[41] Samuel J. Eldersveld, *op. cit.* It is very unfortunate that Eldersveld's excellent study has not been followed up by comparable studies of party organization in various jurisdictions. If it had, we would be able to say much more about most of the problems discussed in this paper. One of the major failures of American political science is the failure to replicate studies and develop knowledge that is cumulative.

[42] Anthony King, "Political Parties in Western Democracies: Some Skeptical Reflections," *Polity*, 2 (Winter, 1969), pp. 111–141.

[43] Leon Epstein, *Political Parties*, p. 8.

[44] Allan P. Sindler, *op. cit.*, p. 101.

Eldersveld goes further: It is no accident, he asserts, that political parties lack the "... conventional attributes of the bureaucratic system: in particular, the bureaucratic prerequisites of impermeability, depersonalized human relations, strict devotion to regulations and rule enforcement, precise allocation of obligations, duties, and roles, discipline and sanctions, even low circulation of personnel...." Their very functions make them "clientele oriented structures," "porous," "permeable and adaptive," "constituted to maximize conflict avoidance," permitting conflict to be "managed, channeled, avoided," "representing and exploiting multiple interests." Structurally the party is a "stratarchy" or a "reciprocal deference structure" lacking a unified command, but characterized by balkanization, "tolerance of autonomy, local initiative, and local inertia." There is a high turnover of party elite, a process "of genuine renovation, adaptation, and reconstitution of the sub-coalitional balance of power within the party structure."[47] Parties in the American system have these properties, Eldersveld argues, because the party is "a specialized system of action in democratic societies, with a special meaning and purpose in the political order."[48] He suggests

> ". . . that an internal logic about the system of party structure in a democratic system *does* exist, which theoretically differentiates it from other social structures. Two common elements in democratic societies contribute to the existence of party groups with these inherently unique structural styles: political power rests on votes; and groups compete for power. Where these components of the democratic system are strong, party groups will manifest their stylistic attributes maximally, where the components are weak, party attributes are also weak."[49]

Detroit is not the world, but students of parties elsewhere have made similar, if less systematic and comprehensive, observations, and they have observed the tendency of party competition to have the effects Eldersveld noted.[50] The APSA Committee was concerned with "keeping the parties apart." But political development in the Western European and Anglo-Saxon democracies indicates that it is not only in America that parties seek the middle ground where majorities are most often found. The broadly based, loosely organized, clientele-oriented, national party attempting to maximize its appeal by rounding off its ideological edges has proliferated throughout the democratic world, casting grave doubts on Duverger's notion that programmatic, bureaucratic membership parties are more "modern" than their more amorphous counterparts.

"So it doesn't whistle"

But, we may ask, so what? Suppose Duverger and the APSA Committee were wrong about the programmatic parties being the wave of the future. Suppose the Committee was wrong about the potential for system reform through party reform. Suppose the Committee was wrong about the sources of cohesion, or the degree of party differences in the British system. Suppose the Committee was wrong about the consequences for party strength of the direct primary or of intraparty democracy generally. Suppose the Committee was mistaken in all its facts about parties and the possibilities of particular reforms. Suppose all this—would it destroy the Committee's normative position? Do the last few decades of research into how parties operate tell us how they *should* operate? What are the implications of this research for a normatively oriented theory of parties? Is the empirical work just a bulwark for the *status quo?*

These questions are important, and their answers not self-evident. Plato felt constrained to concern himself with the practicality of his political teaching. Aristotle felt justified in discarding Plato's proposal for the Guardians' community of property, wives, and children on grounds that it would never work. Yet, thoughtful men know that ideals are not ultimately discredited because of mortals' failure to realize them and are not discarded because of their impracticability—as every world religion demonstrates.

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[45] The downgrading of parties is probably part and parcel of the rise of social determinism in political science. Incidentally, a good case can be made that it is social determinism rather than behavioralism that has encouraged an "all is probably for the best," "conservative" attitude of much recent political science.

[46] Avery Leiserson, *Parties and Politics: An Institutional and Behavioral Approach* (New York: Alfred A. Knopf, 1958), p. 140, my emphasis. Leiserson's book is stimulating for all interested in a theory of parties.

[47] Eldersveld, *op. cit.* pp. 7–12. This simplified and condensed statement of Eldersveld's outline of the structural properties of parties does not do it justice.

[48] Eldersveld, *op. cit.*, p. 4.

[49] Eldersveld, *op. cit.*, p. 12, my emphasis.

[50] It is unfortunate, as I noted above, that we do not have more studies testing the Eldersveld model. We could then speak much more authoritatively about the relations of party organization to system characteristics.

Democratic potential is not necessarily contained in democratic practices as of this or any other moment. In fact, the twentieth century has been filled with political forms unanticipated in the nineteenth. We cannot discard the Committee's goals merely because there exists no example of a system in which they have been realized. But, if goals are not discredited because they have not been realized, *plans* may be discredited because they are incapable of implementation. We should discard a poor plan as we would throw away a road map that is misleading about which road leads where.

It is not always easy to distinguish analytically or existentially between goals and plans for achieving them. The tendency of means to achieve intrinsic value for their proponents complicates the psychological means-end relationship. A nihilist is, after all, one for whom the mode—destruction of what is—has gained intrinsic value. A collectivist, or exponent of laissez-faire, may be one for whom method has achieved value independent of the goal for which it once was seen as a means. The distinction between goals and projects for achieving them, however, should be clearer in philosophy—and political philosophy—than in life. It should clearer in the Committee Report than in any given reformer or given defender of the status quo. Political scientists, of all people, should be clear about such matters; one characteristic of a normatively sound political science is that it makes clear these distinctions.

Unfortunately, the Committee Report, so often reproached for being normative, is in fact normatively deficient. It can be argued that the Committee confused its means and its ends, that it confused the substance of democracy with particular practices and devices for achieving it. Sartori asserts that confusion of the essential character of democracy with the technical means of achieving it is characteristic of the Anglo-Saxon doctrine of democracy.[51] It is possible that the Committee intended to define democracy in terms of the *operations* through which it could be realized. But in this case the concepts would be purely empirical, the definition constituting nothing more than prediction subject to empirical test. I doubt the Committee intended any such thoroughgoing operationalism. A sympathetic reading of the Report suggests that the Committee affirmed as a central value a conception of democracy permitting more self-government (more participation) than did the existing American form. Efficiency was also an independent value of the Committee, but enhanced self-government was, I think, its central goal. The Committee expounded what in more contemporary terms would be called a participatory model of democracy and did so by stipulative definition.

It is interesting that, as normative political science, no effort whatsoever was made to clarify or justify this norm. Apparently its benefits were presumed to be self-evident and perhaps they were and are so for most Americans. But what is commonly acceptable is not necessarily good scholarship. A normative document should identify, examine and justify the norms espoused. Otherwise, the norms are merely arbitrary. While it is not necessary or possible for most of us to achieve Plato's excellence in clarifying and justifying political norms, we are not thereby relieved of the obligation to try, the more so when the norms espoused are controversial. Certainly the Committee was aware that the norm of participation advocated in the Report was controversially rationalistic. The norm of participation advocated by the Committee did not look toward a more complete participation in the rational aspects of government. Increased rationality of political life turns out in the Report to be a value as important as participation, though there is no necessary relation between the two. Still participation and rationality are enduring political norms. The failure to justify them, to investigate relations between these and other values of political life, to ask and answer the question "Who wants it and why should they?" makes the normative aspects of the Report ultimately arbitrary—though not ultimately invalid.

Most of the Report, of course, was not normative but instrumental. Most of it made recommendations based on a belief that they would, if implemented, maximize the stipulated values. Party government clearly has instrumental, not intrinsic, value. It was (and is) desired because of the belief that it would enhance the responsibility of the governors to the governed. If it were shown that some other set of institutional arrangements produced a higher degree of participation and accountability, then they should be preferred. Or—and this is more germane—if it could be shown that some alternative institutional practices resulted in equal accountability, there would be no reason to prefer party government.

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[51] Giovanni Sartori, *Democratic Theory* (Detroit: The Wayne State University Press, 1962), pp. 237–241.

Virtually all of the Committee's recommendations (programmatic parties, intraparty democracy, closed direct primaries, National Policy Councils, National Convention reform), are subject to empirical tests. The value of these recommended reforms is directly and entirely dependent on their ability to produce desired ends. This is why it matters if, in fact, intraparty democracy on policy questions tends to break down rather than build consensus. This is why it matters if nomination by direct primary tends to fracture a party. This is why it matters, also, whether there exists or ever existed a system which demonstrates the postulated connection between devices and outcomes. If no such empirical referent exists, validity of the norms is not destroyed, but the credibility of recommended plans for achieving them through particular institutional practices is importantly affected. The plan is more thoroughly undermined if it can be demonstrated that the recommended practices, in fact, have tended to produce consequences different from those predicted in the recommended model.

Herein lies the importance of the Committee's failure to consider the range of normative and empirical consequences that might follow on its recommendations. No cognizance was taken of potentially undesirable side effects. The high incidence of programmatic parties in multi-party systems was not considered, neither was the tendency of multi-party systems to produce coalition governments in which there is even less party responsibility than in the U.S. The Committee considered neither the durability of purely programmatic parties nor the effects on local representation of centralized parties. The range of possible consequences of the proposed recommendations was simply ignored as though the recommendations were certain to work as predicted. No such certainty existed.

Finally, there was no consideration of values associated with the extant system. On the contrary, the Committee apparently assumed that no intrinsic values were maximized by American institutional practices. They assumed that these institutional practices were deformed, dysfunctional approximations of proper democracy. Not considered were the possibilities that, in a far-flung heterogeneous nation, decisions might best be made through continued aggregation of interests in the legislature; that, in a heterogeneous nation with multiple issues in every election, it might make more sense to hold individuals rather than parties responsible; that individual accountability to voters might make for better representation of the district; that loose parties might be the only institutions consistent with diffuse decision making, itself a protection against error and tyranny; and that Americans might generally prefer to risk the delays of diffuse decision making than the mistakes of easily mobilized majoritarianism. "Who wants it?" was a question never asked by the Committee.

The Committee was thus guilty of both normative slovenliness and empirical inaccuracy. Neither of these shortcomings destroys the validity of its goals. Both cast deep doubt on the reliability of the Committee as a guide to the political Promised Land. We cannot be certain whether that Land is as described by the Committee. We do know that its map is as likely to lose us in the wilderness as to lead us through it.

Part II

The Committee Report: Political Science as Policy Science

Rereading "Toward a More Responsible Two-Party System" in 1970 is more like reading a political campaign document or program for political action than the report of a committee of scholars. In fact, reading the reports issued by the Rules Commission of the Democratic National Committee, chaired by Representative James G. O'Hara[52] or the reports issued by the Commission on Party Structure and Delegate Selection, chaired by Senator George S. McGovern,[53] is more like reading political science than reading the APSA Committee Report. If this is so, as I believe it is, the interesting questions are: Why is it so? What can we learn either about politics or about political science by examining this difference? What can we learn about the behavior of politicians and political scientists by looking at how they behave when concerned about party reform? What relevance does the APSA Committee Report have to the perennial problem of the proper role of the scholar in matters of public policy and politics?

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[52] *Issues and Alternatives: A Study Guide of Democratic National Convention Procedure and Practice* (Washington, D.C.: DNC, October, 1969). Also, see the memoranda "To All Concerned Democrats" sent out by O'Hara in 1970 and 1971.

[53] *Mandate for Reform* (Washington, D.C.: DNC, April, 1970). For information on the Commission and its work, see Andrew J. Glass and Jonathan Cotlin, "Democrats Reform Drive Falters as Spotlight Shifts to Presidential Race," *National Journal,* June 19, 1971. Austin Ranney, a member of the Commission, is now engaged in a study of the Commission's work and its effect on the 1972 Democratic presidential nomination.

I propose to de-emphasize the first three of these questions, not because they are unimportant but because I do not feel competent, without much further investigation, to deal with them more than incidentally. I want to direct my attention instead to the last of the questions, looking at the Report and activities of the APSA Committee as an example of the relation of the discipline and profession to problems of public policy. The debate over the responsible party system, of which the APSA Committee and its Report are simply one episode, offers an excellent opportunity for a case history of the political scientist as policy scientist. This is so because the debate spans the whole history of political science as a separate discipline in the United States: from Wilson, Lowell, Goodnow, and Ford—all presidents of the APSA—through the writings of Herring, Holcombe, and Schattschneider (also all Association presidents), the APSA Committee and its Report, and more recent works of Burns, Bailey, Epstein, Eldersveld and others to the O'Hara and McGovern Commissions.[54]

Further, the Report and the debate about responsible parties deal with subject matter that clearly falls within the political scientists' field of special expertise. They deal with the character of the decisional system, with what Salisbury has called "Constitutional policies." These are matters with which the data and theory of political science are, or should be, most directly concerned, if they are to be concerned about policy at all. Political scientists should be able to provide at least some information about the impact of alternative institutions and practices on policy outcomes and of the consequences of various policy outcomes for the character of the decisional system. Let me then turn back to the Committee Report and examine it again from this perspective.[55]

Both the materials issued by the two Commissions of the Democratic National Committee and the Report of the APSA Committee have the same objective: reform of some aspects of the American party system. To be sure, the APSA Committee had broader concerns than the Commissions, but the work of the Commissions is directly relevant to some of the proposals of the APSA Committee. We have already noted that, in describing its purpose, the APSA Committee says:

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[54] For a most valuable examination of the early history of the debate, see Austin Ranney, *The Doctrine of Responsible Party Government* (Urbana: The University of Illinois Press, 1968). For some of the relevant work referred to above, see Woodrow Wilson, "Cabinet Government in the United States," *The International Review,* 7 (August, 1879), pp. 146–163; "Committee or Cabinet Government," *Overland Monthly,* Series 2, Vol. 3 (January, 1884), pp. 17–33; *Congressional Government* (Boston: Houghton Mifflin and Co., 1885); Henry Jones Ford, *The Rise and Growth of American Politics* (New York: The Macmillan Co., 1898) and his review of Goodnow's Politics and Administration in *The Annals of the American Academy of Political and Social Science,* 16 (September, 1900), pp. 177–188; A. Lawrence Lowell, *Essays on Government* (Boston: Houghton Mifflin and Co., 1899); *Governments and Parties in Continental Europe* (Boston: Houghton Mifflin and Co., 1896); *Public Opinion and Popular Government* (New York: Longmans Green and Co., 1913); Frank J. Goodnow, *Politics and Administration* (New York: The Macmillan Co., 1900). For a full bibliography of Wilson, Ford, Lowell and Goodnow, see the Ranney volume cited above, pp. 165–172. Pendleton Herring, *The Politics of Democracy* (New York: W. W. Norton and Co., 1940). E. E. Schattschneider, *Party Government* (New York: Farrar and Rinehart, 1942); *The Struggle for Party Government* (College Park, Md.: The University of Maryland Press, 1949); *The Deadlock of Democracy* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1963). Stephen K. Bailey, *The Condition of our National Parties* (New York: The Fund for the Republic, 1959). Arthur N. Holcombe, *Our More Perfect Union* (Cambridge: The Harvard University Press, 1950). Samuel J. Eldersveld, *Political Parties, A Behavioral Analysis* (Chicago: Rand McNally and Co., 1964). Austin Ranney and Willmoore Kendall, *Democracy and the American Party System* (New York: Harcourt, Brace and Co., 1956), Ch. 22. Frank J. Sorauf, *Political Parties in the American System* (Boston: Little, Brown and Co., 1964), Ch. 9. William Goodman, *The Two Party System in the United States* (Princeton, N.J.: D. Van Nostrand and Co., 1964), Ch. 25. Allan P. Sindler, *Political Parties in the United States* (New York: St. Martin's Press, 1966), Ch. 5. Willmoore Kendall and George Carey, *Liberalism Versus Conservatism* (Princeton, N.J.: D. Van Nostrand and Co., 1966), Ch. 4. Willmoore Kendall, "The Two Majorities," *Midwest Journal of Politics,* 4 (November, 1960), pp. 317–345. Edward C. Banfield, "In Defense of the American Party System" in *Political Parties, U.S.A.,* ed. Robert Goldwin (Chicago: Rand McNally and Co., 1964), pp. 21–39. James Q. Wilson, *The Amateur Democrat: Club Politics in Three Cities* (Chicago: The University of Chicago Press, 1962). For an interesting follow up on Wilson's book, see John W. Soule and James W. Clarke, "Amateurs and Professionals: A Study of Delegates to the 1968 Democratic National Convention" *American Political Science Review,* 64 (September, 1970), pp. 888–898. Herbert McClosky "Are Political Conventions Nondemocratic" *The New York Times Magazine,* August 4, 1968, pp. 10–11, 62–68. Leon D. Epstein, *Political Parties in Western Democracies* (New York: Praeger, 1967). O'Hara Commission, *Issues and Alternatives: A Study Guide of the Democratic National Convention Procedure and Practice* (Washington, D.C.: The Democratic National Committee, October, 1969); McGovern Commission, *Mandate for Reform* (Washington, D.C.: The Democratic National Committee, April, 1970).

[55] For an excellent discussion of the difference between "constitutional" policy and policy in other areas, see the essay by Robert H. Salisbury, "The Analysis of Public Policy: A Search for Theories and Roles" in Austin Ranney, *Political Science and Public Policy* (Chicago: Markham Publishing Co., 1968), Ch. 7.

> "The purpose of this publication is to bring about fuller public appreciation of a basic weakness in the American two-party system. . . . (It) is not a research document aimed at professional readers only. It seeks the attention of everyone interested in politics . . . the most important thing is effective remedy. . . . It is essential to reach the ears of many citizens. . . . (The Committee centered) its attention on the condition and improvement of national party organization. This emphasis has governed the work of the Committee since its inception. The same emphasis is manifest in the Committee's report" (Foreword, pp. v–ix).

Thus the Committee made quite clear the political or policy purpose of its work and its Report. The Committee clearly saw its work as therapeutic; it aimed to diagnose the ills of the American system and prescribe remedies. It also saw the patient as dangerously ill. The two Commissions of the Democratic National Committee were charged with the same task.[56]

The legitimacy of the political purposes of the two Commissions is clear. Their functions are, and should be, therapeutic. The appropriate product of their work is a program of action. They are performing the functions of the politician. Can the same be said of the APSA Committee? It was the creature of a scholarly, not a political, organization. It was made up of scholars. Was its function scholarly or political? What should have been its function? It is important, at the outset, to make clear that we are comparing the politician not with the scholar in his role as citizen but with the scholar in his role as scholar and scientist. Almost no one questions the right of the scholar to act as a citizen, to have political views, to take positions on policy matters, to vote, to run for office, and to campaign for others. To be sure, some raise questions about the tensions this can create between the man as scientist and the man as citizen, and some express concern lest a man's activities as citizen or politician adversely affect his work as scientist. But the main problems that concern us, and those I wish to pursue, relate to the scholar and scientist and his policy role *as* scholar and scientist.

The problems are not new. (Wilson, Lowell, Goodnow and Ford, as scholars and scientists, advised the American people and politicians how the party system ought to be organized seventy years ago. The Committee on Political Parties of the APSA did the same thing twenty years ago. Schattschneider, Burns and Bailey have continued the same enterprise down to the present.) But neither are the problems simply historical. At the Annual Meeting of the APSA in 1969, David Easton, in his presidential address, "The New Revolution in Political Science," argued that policy engagement is a social responsibility of the intellectual, whatever the institutional form through which it may be expressed. He said:

> "In this post behavioral view, the application of expert knowledge in the service of social reform becomes competitive with the pursuit of knowledge for its own sake. Reform becomes inseparable from knowledge. Clearly there is in birth a *new image* of the professional, one in which science is not necessarily denied its place but in which the scientist is no longer free to divorce the life of the mind from the life of social action. . . . One fact is clear. The crisis of our time spares no group, not even social sciences. . . . For increasing numbers of us it is no longer practical or morally tolerable to stand on the political sidelines when our expertise alerts us to disaster."[57]

It is likely that Woodrow Wilson, Charles Merriam, Hubert Humphrey, Paul Douglas, and Eugene McCarthy felt the same way. They ran for office.[58] In 1964 a group of former presidents of The American Political Science Association endorsed the candidacy of Lyndon Johnson and Hubert Humphrey. Other examples of such action might be cited.[59]

Despite some popular lore to the contrary, political scientists generally have never eschewed participation in the political process, even during the heyday of the behavioral movement. In the essays compiled from plenary session papers given at the 1966 Annual Meeting of APSA, leading spokesmen for the behavioral tendency reaffirmed political scientists' obligation and qualifications to participate in the search—through concern for policy—for the good life. In those essays Almond defined the mission of political scientists as deriving from a "special relation to the problem of violence and coercion in human affairs."[60] Dahl recalled his colleagues to the "ancient obligation of our craft" which he defined as the evaluation of political systems.[61] Deutsch examined the prospects for constructing supra-national communities and, at least by inference, advocated taking the necessary steps.[62] Edward Shils sounded the alarm on the development of a national data center, spelled out the attendant threat to privacy in a clear effort to mobilize political scientists in opposition.[63] Ithiel Pool reminded his readers that the political scientist is often a problem solver, a "political engineer" working at the "tasks of meeting human wants with dispatch and care."[64] Examination of the biographies of leading behavioral political scientists, along with a review of their writings, would definitively bury the notion that behavioral political science involved withdrawal from the political world. Both before and after the Report, many political scientists—well known and unknown, young and old, behavioral and traditional, quantitative and normative—have participated more or less effectively, more or less consciously as actors in the political process. Some have been consultants, some have been advisers, some have written books and articles, some have signed petitions and manifestoes, some have run for office. While advocacy and participation by political scientists have been relatively constant, views about the implications and problems of such participation have varied widely. Some political scientists have simply united their roles as political scientists with their roles as citizens in the manner that the Easton doctrine asserts to be both desirable and necessary. Others have attempted, like Max Weber, to analyze the problems, distinguish roles, and mold their behavior to such distinctions. Still, both intellectual and practical problems persist, and the whole area is likely to be a continuing source of tension, not only within the profession but within individual political scientists.

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[56] See the Preface to the Rules Commission document cited above and the appendix setting forth the origin and mandate of the Commission on Party Structure and Delegate Selection in *Mandate for Reform*, pp. 52–53.

[57] *American Political Science Review*, 63 (December, 1969), p. 1060. Reprinted in the second edition of *The Political System: An Inquiry into the State of Political Science* (New York: Alfred A. Knopf, 1971), pp. 344–345.

[58] Data collected for the last biographic directory of APSA show that approximately 10 percent of those returning questionnaires had run for office; about one-half of those who ran were elected.

[59] At least one former president of the Association, a strong supporter and contributor to the 1964 Democratic ticket, refused to sign the endorsement on the grounds that it was improper to issue such an endorsement as former presidents of APSA. In the spring of 1970, the President, President-Elect and six former presidents of APSA sent Nixon a telegram advising him on Vietnam policy and issued a press release about it. They did so as *political scientists* and identified their official relation to the Association. A week later, seven of the eight issued a press release, on a poll taken of a sample of APSA membership, reporting *political scientists'* views about Vietnam. Both are printed in the *American Political Science Review*, 64 (June, 1970), pp. 589–590.

Since the Report must be judged as policy science, it may be useful, in considering the Report, to recall some examinations of the role of the political scientist in the area of politics and public policy.

Lasswell, without doubt, is the political scientist who has longest and most consistently called on his colleagues to recognize their responsibility to make political science an effective partner in the policy sciences. It is likely that Lasswell's early interest in psychiatry and psychoanalysis, which are concerned with individual therapy, led to his views on social therapy and the policy sciences, to his interest in diagnosing ills in the polity and being prepared to join other disciplines in developing and prescribing remedies, to his attention to the future and his recognition of its intimate ties with the present and past. The therapeutic approach also led him to identify clearly his own concept of a healthy polity. Lasswell always has been committed to a "democratic society, a society in which power and respect are shared, and in which other values are made abundant and accessible to all,"[65] and to the view that knowledge should serve human dignity.

In *Power and Personality,* published in 1948, Lasswell said:

> "The term 'policy' is used to indicate the need of clarifying the social ends to be served by a given allocation (including self allocation) of scientific energy. By laying the accent on policy we are not attacking objectivity. On the contrary, objectivity is put where it belongs, which is in the service of goal values. Policy includes not only breathless short run objectives: policy also comprises intermediate and long range aims and it is in relation to the latter that scientists can make their greatest contribution."[66]

In his essay, "The Policy Orientation," published in 1951, Lasswell said:

> "The policy approach is not to be confounded with the superficial idea that social scientists ought to desert science and engage in full time practical politics. Nor should it be confused with the suggestion that social scientists ought to spend most of their time advising policy makers on immediate questions. . . . We can think of the policy sciences as disciplines concerned with explaining the policy making and executing process, and with locating data and providing interpretations which are relevant to the policy problems of a given period."[67]

Pursuant to this conception of policy science

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[60] Gabriel Almond in Ithiel de Sola Pool, ed., *Contemporary Political Science: Toward Empirical Theory* (New York: McGraw-Hill Book Co., 1967), p. 18.

[61] In Pool, *op. cit.,* p. 179.

[62] In Pool, *op. cit.,* pp. 207–221.

[63] In Pool, *op. cit.,* Ch. 9.

[64] In Pool, *op. cit.,* pp. xii–xiii.

[65] Harold D. Lasswell, *Power and Personality* (New York: W. W. Norton, 1948), p. 9.

[66] Lasswell, *Power and Personality,* p. 122.

[67] Daniel Lerner and Harold D. Lasswell (Eds.), *The Policy Sciences* (Stanford: Stanford University Press, 1951), pp. 7, 8 and 14.

Lasswell has continuously explored different modes of thought to identify how the social scientist can legitimately assist in goal achievement. He sees the intellectual components of the total process as involving at least five kinds of thought. These are: *goal thinking* (determination and clarification); *derivational thinking* (justification of goals or goal values); *trend thinking* (attempting to locate one's self or problem or society in the unfolding course of events, seeking to learn which goals are being realized and to what extent); *scientific thinking* (forming generalizations about the relationship of significant variables in politics and about determinative relationships among goal variables and the variables that condition them); *developmental or projective thinking* (thinking about the future, picturing alternative ways goals are likely to be affected by what we or anyone else may do, invention of alternatives and estimation of their consequences). Such a framework provides a guide to the scholar or scientist; it helps identify those areas in which he can legitimately assist the policy or decision maker.[68]

Ranney, in his essay, "The Study of Policy Content: A Framework for Choice," quotes Don Price's useful differentiation of roles: 1) the *scientific,* which is concerned solely with discovering truth, not with its governmental application or social utility or moral effects, 2) the *professional,* which is concerned with "taking abstractions of science (or other systematic knowledge) and applying them to the concrete practical affairs of men," 3) the *administrative,* whose members may be "prepared to understand and use a wide variety of professional expertise and scholarly disciplines (to help their) political superiors attain their general purposes," and 4) the *political,* whose practitioners "may make use of the skills of administrators, and engineers and scientists," but in the end must "make their most important decisions on the basis of value judgment or hunch or compromise of power interests."[69] Ranney then goes on to say:

> ". . . if political science is in part a true profession, and not just an agglomeration of persons interested in political affairs, then what qualifies political scientists as professionals is their special knowledge and skills, not their common sense, their good will or their passion for social justice. And if that is so, then the political scientist, like any expert, may legitimately speak as a professional on matters of public policy only when what he says rests on and accords with his discipline's special body of knowledge."[70]

In the report *Political Science,*[71] the following warning is given:

> "Whether the researcher seeks personal involvement in the policy process or prefers to observe it from the outside is a matter of individual choice.[72] But whatever role the political scientist may take, his study of the policy process requires a high degree of judiciousness as a norm of *scientific* conduct. He must seek to balance as wisely as he can what his involvement in the policy making process may require. This balancing is far from easy, but it is an *obligation* that both political scientist and public policy maker must appreciate and honor *if political science is to continue as a scientific discipline* and not become the handmaid of special interests. Its task, in short, is to be of service to all and subservient to none." (Emphasis added.)

Another conception of policy science, advanced today by some spokesmen for the "New Left," is radically different from those set forth above. It not only advocates using knowledge to achieve social goals, but also using goals to test the validity of knowledge. This perspective is clear in statements such as: "What we find failing in pluralism is not the content of ideology but the status quo which it served. So long as pluralism was conservative, we rejected it. If it were possible for pluralism to support social change—we doubt it, but it might be possible—we would accept it."[73] This functional view of truth, which sets up political tests for science, is discouragingly familiar in the twentieth century. Unlike the other conceptions outlined here, this one eliminates the tension and duality of the roles of scholar and activist by subordinating science to activism, by politicizing science. This course not only impoverishes science and the scientist but, ironically, may hamper the achievement of the activist's political goals. Orthodoxy can satisfy autocrats without producing desired solutions. Ideological regularity, like love, may not be enough. The roles of scholar and politician are very dissimilar. The scholar's role in the political arena is not only different from, but in many respects antithetical to, his role as scientist. These different role requirements create tensions and problems for political scientists entering the policy realm, including the Committee on Parties when it entered the political arena in the late forties.[74]

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[68] Lasswell, *Power and Personality,* pp. 201–204. Also see the valuable and stimulating article by Heinz Eulau, "H. D. Lasswell's Developmental Analysis" included in Eulau's volume of essays, *Micro-Macro Political Analysis* (Chicago: Aldine Publishing Co., 1969).

[69] Don K. Price, *The Scientific Estate* (Cambridge: Harvard University Press, 1965), pp. 122–135.

[70] Ranney, *Political Science and Public Policy,* p. 5.

[71] Heinz Eulau and James G. March, eds., *Political Science* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1969), p. 43. This book is one of a series prepared in connection with the Survey of the Behavioral and Social Sciences conducted under the auspices of the National Academy of Sciences and the Social Science Research Council.

[72] Obviously this view that the political scientist is "free" to choose a role of policy participant or observer differs importantly from Easton's assertion (and that of the New Left) that engagement is imperative and that "Reform becomes inseparable from knowledge." Easton, *American Political Science Review, op. cit.,* p. 1060, and *The Political System,* (2nd Edition) p. 344.

[73] Marvin Surkin and Alan Wolfe, "The Political Dimension of American Political Science," *Acta Politica,* 5 (October, 1969), pp. 53–54.

By that time the profession had come to no agreed upon norms or behavior patterns. The Committee on Political Parties had many examples but little doctrine to guide it. What then did the Committee do? First, it largely abandoned the discipline, inhibitions and style of science. The Committee said, in the Foreword to the Report (pp. v–vi):

> "It (the Report) is written without regard for the customary form in which scholars present their scientific findings. It does not line up and evaluate every pertinent fact. *It sums up the main facts*. . . . In arriving at the conclusions embodied in this publication, its authors have relied upon methods of analysis they are familiar with through their training and work as political scientists. Though scientific knowledge about the American party system is by no means complete, it was found practical here to build upon available data and insights. The most important thing appeared to be to present the available knowledge in the perspective of its principal inference. . . . *A conscientious effort has been made to get hold of every strand of thinking of the American two-party system.* Although *this is a summation of knowledge, it rests on the results of scientific analysis* that have come from the research activity of a great number of specialists." (Emphasis added.)

Two things are clear from the summary and analysis of the Report in the first section of this paper: (1) The Report was not a summary of available knowledge and insights as of the date of its publication, 1950, and (2) knowledge about parties in 1950 was inadequate on many matters the Committee treated as settled matters of fact. We know far more about political parties today than we did in 1950, and we still do not have answers to some questions the Committee treated as settled.[75]

The Report, which was more a statement of the Committee's judgments about facts and findings than a summary of existing knowledge, did not point out the extent to which many of the Committee's views, at best, lacked supporting evidence and, at worst, were judged by other scholars in the field to contradict existing evidence. For example, the insistence of the Report that intraparty democracy would bring about party unity in support of a comprehensive, coherent program was at odds with the best judgment of the Chairman of the Committee, Schattschneider, who had discussed the matter most effectively in his own writing.[76] Likewise, as noted earlier in this paper, Julius Turner in his "Responsible Parties: A Dissent From the Floor"[77] pointed out that the Committee ignored considerable evidence that party unity and responsibility were already substantially greater on policy questions than the Committee estimated. The Committee ignored defenses of the American party system by such well known scholars as Pendleton Herring. His *The Politics of Democracy: American Parties in Action*[78] was published ten years before the Report, but the Report contains no hint of such a point of view, even though it was widely held then and has since become the dominant view of specialists on parties.[79] Also neglected by the Committee was a short popular defense of the American party system written by Allan Nevins in 1948, two years before the Committee Report was published.[80]

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[74] For excellent discussions of many problems of the policy sciences, see *Policy Sciences: An International Journal* which began publication in the Spring of 1970; also see Yehezkel Dror, *Public Policy Making Reexamined* (San Francisco: Chandler, 1968) and Charles E. Lindblom, *The Policy Making Process* (Englewood Cliffs, N.J.: Prentice-Hall, 1968). For an excellent collection of studies, see Ira Sharkansky, ed., *Policy Analysis in Political Science* (Chicago: Markham Publishing Co., 1970); Sharkansky's opening essay on "The Political Scientist and Policy Analysis" summarizes the contents and sets forth the important problems. Also, see *Knowledge Into Action: Improving the Nation's Use of the Social Sciences* (Washington: National Science Foundation, 1969); *The Behavioral Sciences and the Federal Government* (Washington: National Academy of Sciences, 1968); *The Behavioral Sciences; Outlook and Needs* (Washington: National Academy of Sciences, 1969); *The Use of Social Research in Federal Domestic Programs* (Washington: Government Printing Office, 1967). This study, in four volumes, was done for the Research and Technical Programs Subcommittee of the Committee on Government Operations. An earlier report of interest is *Effective Use of Social Science Research in the Federal Service* (New York: The Russell Sage Foundation, 1950). For an extensive bibliography, see Genevieve Johanna Knezo, *The Social Sciences and Public Policy* (Washington: Legislative Reference Service, June, 1970).

[75] While our knowledge is greater today, we have failed to make our research adequately cumulative and we have failed to test many suggestive hypotheses. For a recent statement on what we do not know about parties, see Anthony King, "Political Parties in Western Democracies," pp. 111–141.

[76] Schattschneider's own work in the parties field is distinguished, and I know that there were aspects of the Report with which he disagreed. Earlier, Henry Jones Ford had taken a view quite different from that of the Committee on the question of intraparty democracy and he was vigorously opposed to the direct primary, though he advocated party responsibility; see Ford's article on "The Direct Primary," *North American Review* (July, 1909), pp. 1–14.

[77] *American Political Science Review*, 45 (March, 1951), pp. 143–152.

[78] New York: W. W. Norton and Co., 1940.

Thus it is clear that the Report conforms neither to its own statement of intent nor to high standards of scholarship. It is not political science, but a campaign document which argues a case, and, like campaign documents, ignores anything contrary to the point of view it is selling.[81]

If the Report does not conform to its own standards or to high standards of scholarship, does it conform to what might be expected of a policy science document? The answer is no, if one accepts the criteria of Lasswell, Ranney, Eulau, and March. On the other hand, it probably meets the criteria of Easton's presidential address. Certainly it is in line with the behavior of a good many political scientists (in and out of the classroom), including some distinguished scholars and former presidents of the Association, from the days of Wilson to the present. All of which confirms the fact that the profession has unsettled problems in this field. It had them in 1950, and it has them today.

Given my low opinion of the Report, the question might be asked: "Why did you sign it?" I have asked myself this question in the last months. Though my opinion of the Report was much less negative then than now, even at that time I had multiple criticisms of the document on both scientific and policy science grounds. *So did every other member of the Committee with whom I discussed the matter, including the Committee's chairman.* The question is: Why did we sign the Report? This question is both interesting and significant, and I believe the answer lies in the dynamics of group interaction.

Groups—committees—especially when they are involved in advocacy, often spawn documents different from and inferior to the views or writings of the separate members. "Groupthink" is inferior, I am convinced, to the product of individual minds, and group efforts to deal with policy questions are almost invariably less skilled in applying scientific knowledge to goal maximization than are individual efforts. Enough is known about small-group interaction to suggest that when a group deals with policy problems, matters relevant to the group's existence but irrelevant to solution of the policy problems come into existence. Achieving consensus may become more important than finding effective solution to the problems. Social norms—"not rocking the boat," not being intransigent, being a good fellow—come into play; so may careerism, unwillingness to alienate influential and respected colleagues, and even the desire to end an interminable committee meeting! Transfer of the principle of majority rule into the area of problem solving (where it does not belong) may lead one to go along with results approved by a majority regardless of one's own opinion of those results. And, of course, laziness and indifference are always at work, particularly in situations that do not seem very important and in which personal reputations are not at stake.

There is a moral here for a profession inevitably concerned with policy problems. Because of the impact of group interaction on effectiveness in dealing with policy problems, it is surely almost always more prudent and more effective to depend for policy advice on individual scientists. Although it may be necessary or desirable to have the advice of many scientists, the results are likely to be better if they are involved as individuals, not as a group. Groups probably should be used when the purposes are primarily organizational or political. A manifesto or report signed by a number of distinguished and well known scholars may have more impact, particularly on the public, than a document authored by a single scholar. But it is less likely to contain reliable advice on how to use existing knowledge to maximize valued goals. If group interactions foreign to problem solving are not to have an adverse effect, the group and its activities must be more carefully structured than is usually the case. The decision seminar, a product of Lasswell's fertile mind, may provide a solution because it specifically structures the group for problem solving.[82]

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[79] Herring's book enjoys so much esteem that it has recently been reissued.

[80] "In Defense of the American Party System," *New York Times Magazine* (July 18, 1948, pp. 5 and following). For further defense of the system in recent years, see Edward C. Banfield "In Defense of the American Party System" in Robert Goldwin, *Political Parties, U.S.A.* (Chicago: Rand McNally and Co., 1964), pp. 21–39; James Q. Wilson, *The Amateur Democrat: Club Politics in Three Cities* (Chicago: The University of Chicago Press, 1962); Herbert McClosky "Are Political Conventions Nondemocratic," *The New York Times Magazine* (August 4, 1968), pp. 10–11, 62–68; Aaron Wildavsky, "On the Superiority of National Conventions," *Review of Politics*, 24 (July, 1962), pp. 307–319; John H. Bunzel, *Anti Politics in America* (New York: Alfred A. Knopf, 1967).

[81] The Report is both far less scholarly and far less effective than the writing of individual scholars who have argued for the same or similar reforms. For example, Schattschneider's *Party Government* is an interesting and extremely influential book, a classic in the field. Other individual scholars who have argued the same case well include James M. Burns and Stephen K. Bailey. See: E. E. Schattschneider, *Party Government* (New York: Rinehart and Co., 1942), James McGregor Burns, *The Deadlock of Democracy* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1963), and Stephen K. Bailey, *The Condition of Our National Parties* (New York: The Fund for the Republic, 1959).

The shortcomings of the APSA Committee Report and the failure of the discipline and profession to clarify more effectively their roles in the public policy field derive from at least four inadequacies: (1) poor handling of value analysis, that is, inadequacies in goal thinking and derivational thinking; (2) failure to explore the interaction of the decisional system and the distribution of values, that is, of policy outcomes; (3) lack of sufficient attention to kinds of thinking peripheral to scientific thinking: goal and derivational thinking (item 1), trend thinking, and developmental analysis; and (4) insufficient empirical theory and inadequate data.

(1) Inadequacies in Goal and Derivational Thinking. *Goal thinking* involves identifying and clarifying the aims of action. Certainly such identification (placing aims and objectives clearly in the forefront of attention) and clarification is a first order of business for the policy scientist. But it is not enough. Such aims and goals must be narrowed so that they are relevant to specific problems and situations. Vague and amorphous statements like those of the APSA Committee: "more responsible," "more effective," "more democratic," "more able to solve great problems" will not serve the purpose. Effective goal thinking cannot be carried on in a global context. The goal to be achieved must be related to concrete patterns of behavior that now shape and distribute the relevant values, that is, to existential contexts that must be modified. The McGovern Commission does this well; the O'Hara Commission also tries to relate alternatives to concrete contexts. Only when this is done can scientific thinking contribute; only when this is done can inconsistencies, conflicts, and possible unanticipated side consequences be revealed. The failure of the APSA Committee to do this job explains in part its failures in scientific thinking, in the invention and evaluation of alternatives, and in the development and recommendation of an adequate research program. I have no doubt, for example, that failure to define goals and relate them to action underlies the Committee's failure to identify the potential conflict between the desire for a coherent, comprehensive program on which the party is united and the desire for intraparty democracy in which an undefined but mass membership makes decisions at all levels about issues and candidates.

Derivational thinking is concerned with the justification of values and is part of the great tradition in political science—from Plato and Aristotle through Cicero, Augustine, and Aquinas to Althusius, Machiavelli, Locke, and Rousseau. The great tradition has always been therapeutic—concerned with the health of the polity, and this, in turn, has always involved an exploration and justification of its meaning and requirements. The great tradition has always been concerned with normative theory, oriented to the inner as well as the outer world of man, to the moral basis of community. The usual methods of empirical science—hypothesis, deduction, observation, experiment—will not give us the normative knowledge required to establish the oughts of political life. The difference is the difference between description and prescription. Take the following propositions: "A political party ought to be responsible to its members," and "The Democratic and Republican parties in the United States are not responsible to their members." It is not too difficult to devise means of testing the second of these propositions, though the APSA Committee merely asserted it. It is harder to validate the first. But again the APSA Committee simply asserted.

Some believe such normative questions are not a concern of political science either 1) because they believe normative questions to be meaningless or 2) because they feel such questions cannot be analyzed by methods appropriate to empirical science. But normative questions have always been, are, and are likely always to remain a major concern of political science. I am convinced that it is both impossible and undesirable for political scientists to avoid dealing in values.

Our discipline is too intimately related to human purposes, too concerned with the institutional practices for achieving them, too involved with evaluating political systems to ignore the values that give political life meaning. Political scientists are not disembodied, value-free spirits with minds like high grade lenses through which reality is filtered but never distorted. Many of our concepts are inevitably value- and affect-laden. Concepts such as "efficiency," "function," "success," "effectiveness," "democracy," and "progress" involve us in value judgments even when they appear to be simply descriptive.[83] Many of our categories—for example, democratic and autocratic—are both normative and descriptive, and there is a continual tension between the normative and descriptive aspects of their meaning.[84]

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[82] For one of Lasswell's most recent articles on the decision seminar, see "The Continuing Decision Seminar as a Technique of Instruction," *Policy Sciences: An International Journal,* 2 (March, 1971), pp. 43–57.

Laboring under the impact of positivism in philosophy and preoccupied by the desire to create an empirical science of politics, many political and social scientists of the 20th century have ignored normative problems and normative theory. In consequence, our normative theory of the 20th century is far less impressive than our progress toward developing an empirical theory of politics. Ignorance of how to identify and deal with the normative dimension crippled the work of the Committee on Political Parties. Believing in the necessity of reform (many members of the Committee had been personally engaged in political activity), the Committee inevitably became involved with normative propositions but was sublimely innocent about how to cope with them except by assertion and exhortation. The unclarified, unevaluated, uncritical, unjustified, normative propositions in the Report reflect the state of the discipline at midcentury. The situation is not much improved today.

(2) Inadequate Value Institutional Analysis. One of the major failures of political science, both in 1950 and 1971, is the failure to explore systematically the relations between political institutions and policy outcomes. In spite of the fact that the literature of political science is full of discussions either assuming or asserting that institutional practices (the party system, interparty competition, intraparty democracy, the direct primary, nonpartisan elections, the unicameral legislature, the federal system, checks and balances, the short ballot, the seniority system, the city manager, or some other institution) have significant consequences for policy, there is little empirical evidence to back up the assumptions and assertions.

An institution, as Lasswell has said, is a "pattern of practices by which values are shaped and distributed. A practice includes operations (such as voting) and perspectives (such as doctrines, slogans, and volitions) connected with voting. Values are basic categories of desired events like power, wealth, and respect."[85] Viewed from this perspective, the political party is a pattern of practices through which values are shaped and distributed. It also is possible, and in some cases likely, that the distribution of values affects the pattern of institutional practices. The interaction is often complex, affected by such intervening variables as the personalities of the participants, system resources, and other socioeconomic factors.

The chief concern of the Committee on Political Parties was with institutions and institutional practices. It was these they proposed to reform.[86] But there is no analysis demonstrating a relationship between institutional practices and policy outcomes, probably because existing scholarship did not provide much basis for such discussion. The literature contained almost no systematic empirical examination of the consequences for value distribution of alternate institutional practices. By defining its goals in institutional terms, the Committee begged the question of the relation between institutions and policy outcomes, between procedures and goals. Therefore, its pseudo-problem solving is unconvincing.

Some later proponents of the responsible party position—especially Stephen Bailey and James Burns—have attempted to spell out the policy consequences of the existing system and indicate what might be expected from responsible party government. Though, in my opinion, the case remains unproved, they have moved the argument beyond the stage of simple assertion and exhortation, which is where the Committee left it, and where most debates about the relation of institutional practices to policy outcomes are likely to remain until there is more systematic research that convincingly demonstrates the impact of existing institutions on policy and the probable consequences of advocated institutional reforms. Today, there are still no studies of which I am aware that establish the ways that value outcomes are affected by party organization or party institutional practices.[87]

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[83] See the very interesting article by David Braybrooke, "The Relevance of Norms to Political Description," *American Political Science Review*, 52 (December, 1958), pp. 989–1006.

[84] For an excellent discussion of this point see Giovanni Sartori, *Democratic Theory*, Ch. 1.

[85] Harold D. Lasswell, "World Organization and Society" in Daniel Lerner and Harold D. Lasswell, *The Policy Sciences* (Stanford: The Stanford University Press, 1951), p. 102. Also, see Harold D. Lasswell, *Power and Personality*, pp. 16–19, and *Politics: Who Gets What, When, How* (New York: McGraw-Hill, 1936).

[86] Banfield makes the same point in his defense of the American party system. He points out that the APSA Committee, like Duverger subsequently, considered the party organization "archaic" and "undemocratic." The Committee said, in effect, that American parties had not kept up with the changing times. See Edward C. Banfield, "In Defense of the American Party System." Banfield makes the point that the critics do not ask whether the party system produces and maintains a good society, they do not evaluate it on the basis of results. This is true of the APSA Committee, though the Committee asserted that the system would not be able to do a good job "in an era beset with problems of unprecedented magnitude at home and abroad. . . ." The Committee made no effort to demonstrate the inadequacies of the parties but asserted that they "have shown little propensity for evolving original or creative ideas about public policy" and have even "been rather sluggish in responding to such ideas in the public interest. . . ." (The Report, p. 17 and p. 15).

The failure of political scientists to pursue exploration of such vital topics is interesting. To be sure, existing efforts make clear that an adequate research strategy and reseach methodology are not easily come by. But this difficulty does not explain why greater efforts have not been made. There is no rule that political scientists must deal only with easy problems. Political scientists have done quite a good job of describing institutional practices, especially in the U.S. and Western Europe; but on the shaping and sharing of values, our record is poor. We have barely begun to study the reciprocal influences of institutional practices and value distributions. But many—perhaps most—of the perennial and central ideological issues turn on these reciprocal influences. Does autocratic leadership produce more rapid gains in the standard of living of masses than democratic leadership? When, where, and at what cost in other values, for example: well being, deference, safety? Do confrontation tactics produce greater or lesser payoffs than action through such legal channels as political parties and elections? Under what circumstances and in terms of which values? How does a high level of civil freedom correlate with progress in mass living standards, with economic development, and so forth? What are the explicit standards of performance by which we might evaluate institutional practices? Questions of this type underlie virtually every policy issue of institutional reform. Apparently, political scientists would rather debate than investigate. However, more systematic investigations, more knowledge about these relations is a clear prerequisite of an effective policy science of politics.[88]

(3) The Emphasis on Scientific Thinking. The emphasis on scientific thinking is a natural concomitant of an age in which appreciation of science has the status of a popular prejudice. So far, the twentieth century has been the age of science, and the reasons are clear. Science has expanded our knowledge to an unbelievable extent, has greatly contributed to providing a better life, and has served the community. Nonetheless, the emphasis on science and scientific method, which has expanded continuously since Galileo and Descartes, has led to a de-emphasis of other kinds of thinking. Goal and derivational thinking have suffered. Concern for trends as instruments for evaluation has suffered. Projection of the future, the invention of alternatives have suffered. There are many museums of the past; I know of no museums that explore alternative pathways of the future. The neglect of these ways of thinking which lie on the periphery of conventional scientific thought has limited our perspective and lessened our skill in goal achievement.

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[87] Some studies that are relevant to this general discussion are: Richard E. Dawson and James A. Robinson, "The Politics of Welfare" in *Politics in the American States*, eds. Herbert Jacob and Kenneth N. Vines (Boston: Little, Brown and Co., 1965), Ch. 10; Thomas R. Dye, "Malapportionment and Public Policy in the States," *The Journal of Politics*, 27 (August, 1965), pp. 586–601; Richard I. Hofferbert, "The Relationship Between Public Policy and Some Structural Variables in the American States," *American Political Science Review*, 60 (March, 1966), pp. 73–82; Douglas W. Rae, *The Political Consequences of Electoral Laws* (New Haven: Yale University Press, 1967); Thomas R. Dye, *Politics, Economics and the Public: Policy Outcomes in American States* (Chicago: Rand McNally and Co., 1966); Otto A. Davis, M. A. H. Dempster, and Aaron Wildavsky, "A Theory of the Budgetary Process," *American Political Science Review*, 60 (September, 1966), pp. 529–547; Phillips Cutwright, "Political Structure, Economic Development and National Security Programs," *American Journal of Sociology*, 70 (March, 1965), pp. 537–551; Sharkansky, *Policy Analysis in Political Science.* In a recent study, Ira Sharkansky and Richard I. Hofferbert, "Discussions of State Politics, Economics, and Public Policy," *American Political Science Review*, 63 (September, 1969), pp. 867–879, the authors assert that until recently the answer was "yes" to the question "Do state political systems leave a distinctive imprint on patterns of public policy?" but that "More recent research has led to a qualified but increasingly confident 'no'." Closely related and valuable for policy analysis are: David Braybrooke and Charles E. Lindblom, *A Strategy of Decision: Policy Evaluation as a Social Process* (Glencoe: The Free Press, 1963) and Charles E. Lindblom, *The Intelligence of Democracy: Decision Making Through Mutual Adjustment* (New York: The Free Press, 1965); in Chapter 20, Lindblom—applying his analysis of decision making through mutual adjustment to the question of party discipline—concludes that "certain ubiquitous forms of non-central decision making and coordination are useful and undervalued procedures" and "are powerful instruments for intelligent policy making. . . ." As Lindblom makes clear, we need much further work in this area.

[88] An interesting discussion of the problems of evaluating political systems and the inadequacies of political science for doing so is: Robert A. Dahl, "The Evaluation of Political Systems" in Ithiel de Sola Pool, *Contemporary Political Science: Toward Empirical Theory*, Ch. 5. This whole volume is important to anyone interested in the questions I have discussed.

A principal limitation to the development of policy science derives from the extent to which science has oriented our thinking to the past and away from the future. The policy maker is always future oriented. So is the decision maker. Decision makers are interested in the past only as a guide to the future. Politicians, likewise, are future oriented. Most political scientists are oriented to what exists. Their data concern existing and past political systems. Their questions relate to what has happened. The failure of political scientists to be seriously, intellectually concerned with the future has drastically limited their effectiveness as policy scientists. I am not speaking here of prediction. Prediction is a part of the method of science, but it deals with small segments of reality. I am speaking of the projection into an emergent future of those broader wholes with which the policy maker must deal. The most stimulating and suggestive attempts at providing intellectual tools for this task are those of Lasswell. Bertrand de Jouvenel also has grappled in imaginative ways with the problems of the future.[89] But almost no books on methodology in political science (or, for that matter, the social sciences) refer to either Lasswell's or Jouvenel's work that is most relevant to the problems of thinking about the future. Coursework in universities is also almost wholly oriented to the past; perhaps this has something to do with recent student concern about relevance.

Developmental analysis begins by assuming change. At any given point in time, a society has not only an intimate relation to the past out of which it developed but to the future into which it is always moving. Obviously, the problem of the future in traditional societies is less important because change is slow. Today, the rapidity of change—including cultural change—sweeps away social and cultural moorings that anchor individuals in society and dissolves societies and identities. It requires continuously finding means to relocate and reintegrate individuals in their environment. It requires continuous replanning and reestimating consequences to take account of innovation. To accomplish these we need better ways of envisaging the emerging future. We need developmental constructs that will help us see "from what, to what." Developmental constructs are not predictions in the scientific sense. Neither are they trend analyses, which seek to determine the direction of movement and register the strength of the variables that produce the trend. Developmental constructs are tentative and hypothetical. While they must be frankly imaginative, they also must be disciplined by the knowledge of trends and of conditioning factors supplied by science.

Developmental analysis elaborates developmental constructs selected according to goal values. "Knowledge is mustered for clear cut objectives, and is fully related to the most likely contingencies to appear in the unfolding process of history."[90] Developmental analysis seeks imaginatively to tie together the past, the present, and a pattern imputed to the future. It is a creative effort to look at the future as it *might* be, not merely as it is likely to be. As such, it helps guide research, invent alternatives and provide insights to help the policy maker take advantage of future opportunities to maximize goal values.

Viewed from this perspective, the Report of the APSA Committee has serious deficiencies. These are reflected in the inadequacy with which the recommendations were tied to the emerging future. Perhaps this was because the Committee members themselves did not believe in the possibility of implementing the recommendations. The continued nationalization of American politics was projected and has probably occurred. But the Committee did not set forth the context that could have provided alternative maps of an emerging national politics. This failure to specify a spatial and temporal context doubtless helps account for the failure of the only real effort made to implement any significant portion of the Committee's recommendations.

It is particularly instructive to read Daniel Ogden's paper, "Party Theory and Political Reality Inside the Democratic Party," describing the failure of the efforts of Paul Butler, as Chairman of the Democratic National Committee, to institute many reforms recommended by the APSA Committee.[91] Perhaps with more limited objectives and a more realistic view of the emerging future, the McGovern and O'Hara Commissions will be able to do better; they should profit from Butler's experience.

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[89] Bertrand de Jouvenel, *The Art of Conjecture* (New York: Basic Books, Inc., 1967). See also Heinz Eulau's excellent essay "H. D. Lasswell's Developmental Analysis" for a short description of developmental analysis and the developmental construct and their importance to political science: Eulau, *Micro-Macro Political Analysis,* Ch. 4. The magazine *Policy Sciences* is now providing a valuable forum for discussion of these problems.

[90] Harold D. Lasswell, *Power and Personality,* p. 204.

[91] Ogden's paper was prepared for the Annual Meeting of the APSA in New York in 1960 and may be obtained from University Microfilms, Ann Arbor, Michigan.

(4) Insufficient Data and Inadequate Empirical Theory. As the first section of this paper demonstrated, the Committee's work was drastically handicapped by insufficient data and inadequate empirical theory. Today, we still lack reliable answers to many of the questions with which the Committee was concerned. I do not believe that any serious political scientist concerned about the important problems of the day and aware of the existing levels of knowledge can deny that ours is still a data-poor discipline. The number of important, potentially fruitful hypotheses which abound in the party literature today, in the books by Eldersveld, Leiserson, Epstein, Key, Cummings, Ranney, Sorauf, Sindler, Goodman, Bone, Bailey, Schattschneider, Burns, Miller, Converse, Stokes, Campbell, Hinderaker, and others is impressive. The data collection and analysis needed to explore these hypotheses adequately will be expensive and time consuming. But the development and testing of more adequate theory awaits this painstaking work.

The paucity of data and the inadequacy of theory confronting the Committee were matched by its sublime unconcern for the empirical validity of its propositions. The Committee utterly failed to distinguish what it knew from what it guessed or hoped. If the members of the Committee, including myself, were conscious of the dubious reliability of much of our theory our doubts remained a carefully guarded secret. The Committee Report stands therefore as a monument to the inadequacies of the discipline and the failure of the members of the Committee as policy scientists.

I have felt free to judge the Committee's work harshly because I was a member of the Committee and a signer of the Report. I have thought it important to confront its failures directly because I believe those failures have significant lessons to teach us.

Perhaps the most important lesson is simply that the job of the policy scientist is not easy. Policy science is not exhortation. It is not mere advocacy. Policy science involves straddling two worlds. As Max Weber pointed out in two brilliant essays, "Science as a Vocation" and "Politics as a Vocation," there are important differences in goals, experience, habits, skills, environment and temperament of scientists and politicians.[92] Himself attracted both to the academy and to politics, Weber analyzed, with extraordinary insight, the problems of being involved in both. Politicians, he reminded us, are characteristically concerned with wholes, scientists with parts; politicians make their careers within organizations and are accustomed to organizational imperatives, solitude is the natural habitat of the scientist. Politics requires commitment, science demands detachment and dispassion. The scientist is free from the time pressures that require the politician to act now, and from decisions that have concrete, not abstract, consequences. Each of these differences complicates communication between policy maker and political scientist. Each diminishes the likelihood that one can successfully perform in the other role. The Committee underestimated the difficulties. It conformed precisely to Eulau's description of "the older political science" which, "inordinately preoccupied with practical problems of governmental reform, tended to minimize the distinction between the practical and the scientific."[93]

The political scientist can be successful as policy scientist, not by abdicating his role as scientist and abandoning his tools as did the APSA Committee on Political Parties, but by succeeding in his own tasks, sharpening his own tools, and thoughtfully applying his distinctive knowledge and skills to the problems at hand. "After all, social science can inform politics only to the extent that it can inform at all—i.e., only to the extent that it has actually been developed."[94]

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[92] The two essays are reprinted in H. H. Gerth and C. Wright Mills, *From Max Weber, Essays in Sociology* (Fairlawn, N.J.: Oxford University Press, 1946). Also see the stimulating article by Harry Eckstein, which discusses Weber at length: "Political Science and Public Policy" in Ithiel de Sola Pool, *Contemporary Political Science*, Ch. 5; and the stimulating article by Duncan MacRae, Jr., "Scientific Communication, Ethical Argument and Public Policy," *American Political Science Review*, 65 (March, 1971), pp. 38–50.

[93] In Pool, *op. cit.*, p. 57.

[94] Harry Eckstein, in Pool, *op. cit.*, p. 155.

The Silent Revolution in Europe: Intergenerational Change in Post-Industrial Societies*

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Economic Scarcity and Political Priorities: An Analytic Framework

A transformation may be taking place in the political cultures of advanced industrial societies. This transformation seems to be altering the basic value priorities of given generations, as a result of changing conditions influencing their basic socialization. The changes seem to affect the stand one takes on current political issues and may have a long-term tendency to alter existing patterns of political partisanship. In this article, I will present evidence based on surveys from six countries concerning these processes.

The findings seem to support a specific interpretation of the causes of value change in post-industrial societies; let me first outline this interpretation. My basic hypothesis is that given individuals pursue various goals in hierarchical order—giving maximum attention to the things they sense to be the most important unsatisfied needs at a given time.[1] A man lost in a desert, for example, may be obsessed by his need for water, devoting virtually all his attention to the search for it. When a supply of water is readily available but food is scarce, he may take the former need for granted (having achieved biological homeostasis in that respect) and may devote himself to gathering food. Once his food supply has reached a subsistence level, an individual may continue striving in order to pile up a comfortable margin of economic security; later, he may gradually shift his focus, coming to desire worldly goods as symbols of affluence—more in order to enhance his status among less affluent acquaintances than for the utility of the goods themselves. In a sense, however, the pursuit of symbols of affluence could be regarded as derivative from the search for sustenance.

Important groups among the populations of Western societies have passed beyond these stages, we believe, and today are acting in pursuit of goals which (unlike symbols of affluence) no longer have a direct relationship to the imperatives of economic security.[2] These individuals—drawn largely from the younger cohorts of the modern middle class—have been socialized during an unprecedentedly long period of unprecedentedly high affluence. For them, economic security may be taken for granted, as the supply of water or the air we breathe once could.

If this hypothesis is correct, it suggests that intergenerational political conflict is likely. We would expect to find such conflict if it is true that individuals have a tendency to *retain* a given value hierarchy throughout adult life, once a basic character has been formed during childhood and youth. An illustration would be the miser who experienced economic hardship during his childhood, saw hard work and frugality as a way out, and continued accumulating frantically long after his economic needs had been assured. This is, no doubt, an extreme case, but considerable evidence suggests that people do tend to retain early-instilled preferences. Drawing on the work of Abraham Maslow,[3] we reason that the age cohorts who had experienced the wars and scarcities of the era preceding the West European economic miracles would accord a relatively high priority to economic security and to what Maslow terms the safety needs. For the younger cohorts, a set of "post-bourgeois" values, relating to the need for belonging and to asthetic and

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* The author is indebted to Samuel Barnes, Karl Deutsch, Kent Jennings, Warren Miller, Robert Putnam, and Donald Stokes for comments and criticism of an earlier draft of this article.

[1] For a more complete presentation of this hypothesis, see Ronald Inglehart, "Révolutionnarisme Post-Bourgeois en France, en Allemagne et aux États-Unis," *Il Politico*, 36, 2 (1971) 209–238; and Ronald Inglehart and Leon Lindberg, "Political Cleavages in Post-Industrial Society: the May Revolt in France" (forthcoming).

[2] An example of induced *reversion* to biological priorities, under starvation conditions, is described in James C. Davies, *Human Nature and Politics* (New York: Wiley, 1963), p. 13. A conscientious objector taking part in an experiment progressively lost his interest in social welfare work after a number of weeks on a semistarvation diet.

[3] See Abraham H. Maslow, *Motivation and Personality* (New York: Harper, 1954). An excellent discussion of value hierarchies and their political implications appears in Robert E. Lane, *Political Thinking and Consciousness* (Chicago: Markham, 1970), Chapter 2.

intellectual needs, would be more likely to take top priorities.[4]

Probably the best documented evidence of the persistence of early-instilled political preferences is found in the area of political party identification.[5] But it is precisely in this area that our hypotheses have another interesting implication—they suggest the presence of a long-term pressure acting to reshape previous relationships between social class and political party preference. If the shift to a new set of value priorities results from attainment of a saturation level in regard to needs previously given top priority, we would expect a new ordering of values to manifest itself first and most fully among those groups that have attained the highest levels of affluence. In other words, we would expect to find it appearing first among the upper middle class, and among working class or farm groups only after a considerable delay. But despite the fact that middle-class status has generally tended to be associated with a preference for relatively conservative political parties, the newly emerging type of value priorities seems likely to be linked with support for radical social change. Under given conditions, we believe, this can lead to massive shifts to the political parties of the Left on the part of younger middle-class groups. Conversely, working-class respondents would be relatively likely to have underlying value preferences which make them potential recruits for conservative parties—despite their traditional association with parties of the Left. These individuals have attained a certain level of prosperity relatively recently, and apparently continue to place a comparatively high value on defending and extending their recent gains. Paradoxically, although they have working-class occupations, they may manifest what is sometimes regarded as a "bourgeois" mentality.

In short, the "middle majority"[6] hypothesis may have been correct, as far as it went: increasing affluence would make the working class feel they had a stake in the system. By comparison with the emerging post-bourgeois group, both the proletariat and bourgeoisie of industrial society shared certain acquisitive values; their conflicts were not due to differences in basic value priorities, but to the fact that one party *had,* and was overwhelmingly eager to keep, what the other party *wanted* above all. If this were, indeed, the case, an increasing degree of property ownership might well "embourgeoisify" the workers, lessening the intensity of class conflict. Nevertheless, Western societies do not seem to have reached a new era of consensual politics: the emergence of "post-bourgeois" value priorities among a small but critical sector of these societies may lead to a phase during which political cleavages will no longer be based primarily on the familiar economic conflicts—but will, increasingly, be polarized according to differences in underlying value priorities.[7] This new axis of political cleavage would, initially, oppose one section of the middle class to the remainder of society. Assuming continued prosperity, however, our analysis suggests that this deviant group would grow in relative size.

In a recent article,[8] the outcome of the French 1968 uprising and elections was interpreted on the basis of the foregoing conceptual scheme. The May Revolt, we argued, was an event which had an exceptionally powerful impact on the French electorate, causing many voters to re-examine their habitual party preferences in the light of underlying values—and to realign themselves accordingly. Although the prevailing rhetoric of the May Revolt cast it as the movement of an exploited proletariat rising against bourgeois Gaullist oppression, in the subsequent elections the French working class showed a net shift which favored the Gaullists—while the modern middle class[9] (especially

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[4] Supporting evidence might be drawn from Richard Flacks' study of political activists and nonactivists among University of Chicago students. His findings indicate that students from relatively affluent homes tend to place greater emphasis on involvement in intellectual and esthetic pursuits, humanitarian considerations, and opportunities for self-expression, and they tend to de-emphasize material success, personal achievement, conventional morality, and religiosity; moreover, they are much more likely to become activists than students from less affluent backgrounds. See Richard Flacks, "The Revolt of the Advantaged: An Exploration of the Roots of Student Protest," *Journal of Social Issues,* 23 (1967).

[5] See, among others, Angus Campbell, Philip Converse, Warren Miller and Donald Stokes, *The American Voter* (New York: Wiley, 1960). Cf. Philip Converse and Georges Dupeux, "Politicization of the Electorate in France and the U.S.," in Angus Campbell *et al., Elections and the Political Order* (New York: Wiley, 1966), Chapter 14.

[6] This line of reasoning is presented in Ralf Dahrendorf, "Recent Changes in the Class Structure of European Societies"; and in Seymour Lipset, "The Changing Class Structure and Contemporary European Politics," both in *A New Europe,* ed., Stephen Graubard (Boston: Beacon, 1967).

[7] Joseph Schumpeter reasoned along somewhat similar lines in *Capitalism, Socialism and Democracy* (New York: Harper, 1942).

[8] See Inglehart, *op. cit.*

[9] We distinguish between the modern middle class and the traditional middle class on the basis of occupation: the latter group consists of self-employed small businessmen and artisans; the former group comprises people with nonmanual occupations in the modern sector of the economy, and tends to be characterized by a higher level of economic security (and a lower

its younger members) showed a net shift to the Left, by comparison with the way these groups had voted in 1967. They apparently did so, in part, because the younger middle class tended to place a lower value on economic security and domestic order than did the workers. The disorders of 1968—particularly insofar as they entailed destruction of property—seem to have had a negative impact on the working class, driving many of them from their traditionally Leftist political loyalties toward support of General De Gaulle—who was widely seen as the guarantor of order.

Thanks to an ongoing program of public opinion research sponsored by the European Community, it was possible to take a set of predictions based on this interpretation of the 1968 French data and subject them to a more exhaustive cross-national test. Working in collaboration with the European Community Information Service, I took part in the design of a six-nation survey of political change in Western Europe, which went into the field in 1970.[10] Items included in these surveys were designed to tap politically relevant aspects of an individual's basic value hierarchy. We wanted to know which values a respondent would rank highest when he was forced to choose on the one hand between such things as economic security and domestic order (which we regarded as indicating instrumental or "acquisitive" values), and on the other items relating to expressive, or "post-bourgeois" value priorities. Our expectation was that those who had been socialized under conditions of relatively high and stable affluence should show a relative preference for such values as free speech and political participation. In the current social context, it was hypothesized, these values should be linked with a relatively change-oriented stand on current political issues. And if, as hypothesized, we are dealing with a basic, rather than a peripheral, aspect of the individual's socialization, we should find indications that these preferences influence a broad range of his political opinions.

We might expect the emergence of value preferences which do not conform to those of society as a whole to be linked with a preference for change-oriented political parties—in terms of traditional concepts, the parties of the Left. This tendency would be resisted, however, by another aspect of the presumed persistence of early political learning—the tendency toward persistence of early-instilled political party identification. To the extent that given individuals have acquired a sense of identification with the (traditionally middle-class) parties of the Right and Center, they would be slow to shift their support to a party of the Left, even assuming the presence of favorable underlying value preferences. The converse should also hold true; respondents who were raised in a Left-oriented political tradition would normally be somewhat inhibited from shifting to parties of the Right, even assuming the presence of relatively conservative value preferences.

An Empirically-based Typology of Value Priorities and Its Expected Relationship to Economic History

These hypotheses concern changes in value priorities over long periods of time. Very little relevant time-series data is available, and consequently one cannot test this interpretation *directly.* To do so conclusively would require a large-scale research program continuing over several decades. In the meantime, however, one *can* subject these hypotheses to a variety of indirect tests. While these tests cannot provide a definitive validation or falsification, they may aid the reader in forming a judgment concerning the relative plausibility of this interpretation, in the light of the total configuration of evidence.

The first type of indirect evidence is drawn from cross-sectional age-cohort analysis. This approach involves substantial methodological problems. Can one, in fact, draw conclusions about change over time from cross-sectional data? Under some conditions the answer, rather clearly, is yes: it depends on how much confidence one has that the cross-sectional data measure relatively stable characteristics of a given age cohort.[11] To take an obvious example, you can project how many 21-year-olds there will be in the U.S. ten years from now

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likelihood of being attracted to extreme-Right political movements). Our use of this distinction was suggested by Seymour Martin Lipset's analysis in *Political Man: The Social Bases of Politics* (Garden City: Doubleday, 1960), especially Chapter 5.

[10] We are indebted to Jacques-René Rabier, director-general of the European Community Information Service, for the role he has played in encouraging cross-national collaborative research with Michigan (and a number of other universities) over the past several years.

[11] For a sophisticated discussion and application of this type of analysis, see David Butler and Donald Stokes, *Political Change in Britain: Forces Shaping Electoral Choice* (New York: St. Martin's 1969), especially Chapters 3, 11 and 12. Butler and Stokes find that political party affiliation is a rather stable characteristic of British cohorts. In the relatively large swing from Conservative to Labour which took place from 1959 to 1963, they conclude, replacement of the electorate (linked with differential birth and mortality rates) actually played a larger role than did conversion of voters from one party to the other.

Table 1. Educational Level, by Age Cohort

(Percentage educated beyond primary school)

Age range of cohort in 1970	Neth.	Belg.	Italy	France	Germany	Britain
16–24	87%	87	84	77	48	47
25–34	66	69	60	62	39	37
35–44	58	67	43	50	29	26
45–54	44	50	35	39	33	24
55–64	40	35	29	33	28	19
65+	25	17	28	30	23	13

and twenty years from now, if you have data on the size of the various age-groups today. Your prediction might be upset by a major war or other catastrophe, but otherwise it is likely to be fairly accurate. To take another example, let us look at the differences in educational levels among the respective age cohorts in our six national samples. (See Table 1.) The differences are quite sizeable, reflecting the massive expansion of secondary and higher education in Western Europe during the past two generations. These figures, I would argue, reflect a relatively enduring characteristic of the respective age cohorts: except among the youngest group, the level is unlikely to rise much; nor is it likely to decline for any of the cohorts. The presence of a high level of formal education may well have important effects on the political behavior of a given group. To the extent that such relationships can be demonstrated, longitudinal projections derived from the age-cohort differences are likely to be reasonably reliable.

With these remarks in mind, let us examine the pattern of responses to a series of items which were designed to measure an individual's hierarchy of politically relevant values. Representative national samples of the population over 15 years of age in Great Britain, Germany, Belgium, The Netherlands, France and Italy were asked the question:[12]

"If you had to choose among the following things, which are the *two* that seem most desirable to you?

Maintaining order in the nation.

Giving the people more say in important political decisions.

Fighting rising prices.

Protecting freedom of speech."

Two choices only were permitted; thus (aside from nonresponse and partial nonresponse) it was possible for a respondent to select any of six possible pairs of items. In relation to my hypotheses, two of the items (the first and third) were regarded as indicating traditional "acquisitive" value preferences: a concern with domestic order is presumed to relate, above all, to the protection of property;[13] and

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[12] Fieldwork was carried out in February and March, 1970, by Louis Harris Research, Ltd. (London), Institut für Demoskopie (Allensbach), International Research Associates (Brussels), Netherlands Institut voor de Publieke Opinie (Amsterdam), Institut français d'opinion publique (Paris), and Institut per le Ricerche Statische e l'Analisi del'opinione Pubblica (Milan). The respective samples had N's of: 1975 (Britain), 2021 (Germany), 1298 (Belgium), 1230 (Netherlands), 2046 (France), and 1822 (Italy).

The survey also included Luxembourg, but the number of respondents from that country (335) was considered too small for use in the present analysis. The Dutch sample has been weighed to correct for sampling deficiencies, and the weighted N appears in the following tables; while the data from The Netherlands are, in the author's opinion, less reliable than those from the other countries, the crucial intra-sample differences discussed in this article are sufficiently large as to minimize the likelihood that they simply reflect sampling error. On the other hand, cross-national comparisons based on the Dutch marginals should be viewed with reservations. The surveys in the European Community countries were sponsored by the European Community Information Service; research in Great Britain was supported by funds from the University of Michigan.

[13] From the viewpoint of most of our respondents, that is: in extreme situations, threats to domestic order can, of course, involve danger to one's life. To the extent that a concern with one's personal safety *is* involved, the item taps the need which Maslow places immediately below the economic needs in his hierarchy. Post-bourgeois responses, then, are seen as reflecting security in respect to *both* the economic and safety needs. There is reason to expect that the intergenerational pattern of priorities would be similar for the two types of needs: older cohorts are more likely to have experienced threats to their physical security, as well as to their economic security, during formative years. The persisting effect of the former experience is suggested by the fact that older Germans are more likely to express a fear of World War than are the post-war cohorts: see Peter Merkl, "Politico-Cultural Restraints on West-German Foreign Policy," *Comparative Political Studies,* 3 (January, 1971). We doubt that many of our respondents felt physically threatened in 1970, however; for most, this item probably evokes nothing more than thoughts of property damage.

Table 2. "Pure" Value Pairs, by Nation

(Percentage choosing each pair within given national sample)

Pair chosen:	Nether.	Belgium	Italy	France	Germany	Britain
Acquisitive	30%	32	35	38	43	36
Post-bourgeois	17	14	13	11	10	8

the relevance of rising prices to acquisitive motivations is fairly self-evident. The other two items in this set were regarded as indicating a preference for "post-bourgeois" values. I use the latter term with an awareness that political liberties were among the things traditionally valued by the bourgeoisie—but with the conviction that this group was characterized even more distinctively by a predominant concern for acquiring and retaining economic goods. It is not a question of valuing one thing positively and the other negatively: other items in our data indicate that most people place a positive value on all four of the above goals. But in politics it is sometimes impossible to maximize one good without detriment to another. In such cases, the relative *priority* among valued objectives becomes a vital consideration. Our questions, therefore, were cast in the form of forced-choice items in an attempt to measure these priorities. Empirically, it appears that although nearly everyone strongly favors freedom of speech (for example), there are striking differences in the priority given to it by various social groups.

The choice of one "post-bourgeois" item showed a relatively strong positive correlation with the choice of the other "post-bourgeois" item, in each national sample; the same was true of the two "acquisitive" items. Thus, approximately half of the respondents in each sample chose one of the two "pure" pairs of value preferences, with the other half spread over the four remaining "mixed" (or ambivalent) pairs, plus nonresponse. (See Table 2.) Note that the pure "acquisitive" pair predominates across the six samples by a ratio of at least 3:1.

On the basis of the choices made among these four items, it is possible to classify our respondents into value-priority groups, ranging from a "pure" acquisitive type to a "pure" post-bourgeois type, with several intermediate categories. Use of this typology provides a simple, straightforward and intuitively meaningful basis for analysis. I should emphasize, however, that the use of these categories does not rest exclusively on an individual's choices among the four goals listed above. On the contrary, these four were selected as the basis of our typology *because* they seem to constitute a particularly sensitive indicator of a broad range of other political preferences—some of which have a fairly obvious relationship to the four basic items, and some of which appear to be quite distinct, in terms of face content. For example, on the basis of the value pair chosen by a given individual, one can make a fairly accurate prediction of his response to the following item:

"Within the last couple of years, there have been large-scale student demonstrations in (Britain) and other countries. In general, how do you view these? Are you:
—very favorable
—rather favorable
—rather unfavorable
—very unfavorable"

Table 3 shows the respective levels of support for student demonstrations in each of the six countries. While the majority is unfavorable in each country, there is a wide variation in support levels according to the pair of value choices made: a mean difference of fully fifty-five percentage points separates the "acquisitive" and "post-bourgeois" types of respondents. In every country, respondents choosing the pure "post-bourgeois" pair are the group most favorable to student demonstrations, giving a heavy majority in support. Overall, they are more than four times as likely to favor the demonstrations as are the "acquisitive" respondents. With only one exception among the 36 value pairs shown, respondents choosing the pure "acquisitive" value pair are *least* favorable to the student demonstrations (in the one exceptional case, the "acquisitive" respondents are within three percentage points of the least favorable group).

Factor analyses of the respective national samples consistently showed these value choices

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We follow the Marxist tradition in according an important role to economic determination—although *only* within certain thresholds. Both before industrialization and after an industrial society reaches a threshold of general economic security, we believe that other values are likely to prevail more widely. The concept of discretionary income is analogous to our interpretation of the second threshold: as an economy rises well above the subsistence level, even specifically economic behavior can be explained by economic variables to a progressively diminishing extent.

Table 3. Attitude Toward Student Demonstrations, by Value Pairs Chosen

(Percentage favorable to student, demonstrations)

Nation	Order & Prices*	Order & Free Speech	Order & Participation	Prices & Free Speech	Prices & Participation	Free Speech & Participation*	Overall
Neth.	21%	33	42	37	47	70	39%
Italy	19	29	36	42	54	77	36
Belg.	18	29	36	32	60	65	35
Germ.	14	35	29	35	46	83	32
France	12	18	23	38	41	66	27
Brit.	12	22	9	22	60	65	17
mean:	16%	28	29	35	51	71	

* Indicates the two "pure" value pairs, on the basis of our hypothesis—representing, respectively, "acquisitive" and "post-bourgeois" values.

to be among the high-loading items (in a set of 25 variables) on what I interpret as an "acquisitive/post-bourgeois values" factor.[14] In every case, the choice of "order" and "prices" had relatively high negative loadings on this factor, while the choice of "free speech'" and "'participation" had relatively high positive loadings.

In view of the face content of the items, it is not particularly surprising that we find a strong relationship between these value choices and the respondents' support of or opposition to student demonstrations. But these same value choices also show significant relationships with other political preferences which have no obvious similarity in terms of face content. For example, they serve as good predictors of attitudes toward supranational European integration. Table 4 shows the relationship between value choices and responses to a three-item index of support for European integration.[15] Once again, we find the two theoretically "pure" sets of value priorities occupying the opposite poles of the continuum—with post-bourgeois respondents markedly more European in outlook than the acquisitive-type respondents. There are only two mild exceptions to the rule that the respondents choosing the theoretically "ambivalent" value pairs are more European than the theoretically pure acquisitives types, and less European than the post-bourgeois types. The ordering within the "ambivalent" pairs changes somewhat from the pattern we found in Table 3, with a concern for rising prices now showing a stronger association with the negative end of the scale than the preoccupation with domestic order which formerly held that place; in other respects, the ranking of value pairs remains the same. Overall, the post-bourgeois respondents are more than twice as likely to be classified as "clearly

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[14] Other high-loading items on this factor related to: expectations of a higher standard of living, support for student demonstrations, support for radical social change, and support for a variety of proposals for European integration (all of which had positive polarity); and emphasis on job security, pride in one's own nationality, and support for a strong national army (which had negative polarity). Because of limited funds, the British questionnaire was shorter than the one used in the European Community countries, and the factor analysis for that sample omits some of the items available in the larger data sets. Apart from these omissions, the British response pattern seems to parallel that found on the Continent. The fact that expectations of a higher future standard of living seem to go with giving a relatively *low* priority to economic security is interesting: it tends to confirm our interpretation that, for the post-bourgeois group, economic values are relatively unimportant because they are taken for granted.

[15] This index was based on responses to the following items: "Supposing the people of Britain and the Common Market were asked to decide on the following questions. How would you vote . . .?

—Would you be in favor of, or against, the election of a European parliament by direct universal suffrage; that is, a parliament elected by all the voters in the member countries?

—Would you be willing to accept, over and above the (British) government, a European government responsible for a common policy in foreign affairs, defense and the economy?

—If a President of a United States of Europe were being elected by popular vote, would you be willing to vote for a candidate *not* of your own country, if his personality and programme corresponded more closely to your ideas than those of the candidates from your own country?"

A respondent was categorized as "clearly for" European integration if he gave favorable responses to all *three* of these items; or to at least two of them provided that his response to the third item was "don't know," rather than "against." For a much more detailed exploration of this topic, see my article "Changing Value Priorities and European Integration," *Journal of Common Market Studies,* September, 1971.

Table 4. Support for European Integration, by Value Pairs Chosen

(Percentage scored as "Clearly For" on European Integration Index)

Nation	Prices & Order*	Prices & Free Speech	Prices & Participation	Order & Free Speech	Order & Participation	Free Speech & Participation*	Overall
Italy	48%	53	63	65	73	69	57%
Germany	45	57	64	67	59	76	55
France	36	38	48	48	61	69	44
Belgium	31	39	43	46	50	64	42
Netherlands	28	31	34	43	52	62	39
Britain	13	16	16	36	20	32	17
mean:	30%	36	41	47	49	61	

* Indicates the two "pure" value pairs.

for" supranational European integration as are the acquisitive respondents.[16]

As we shall see presently, these value choices also show a rather striking set of relationships with social structure and political party preferences. This is scarcely the sort of pattern which would emerge from random answering or from a superficial response to transient stimuli. It appears that these items tap a relatively well integrated and deep-rooted aspect of the respondents' political orientations.

If these items *do* tap attitudes that are early established and relatively persistent, responses to them should show distinctive patterns, reflecting distinctive conditions which prevailed during the formative years of the respective age cohorts. Our next step, therefore, is to examine variations in response according to age group. Before doing so, let us attempt to specify, as precisely as possible, what sort of pattern we would expect to find on the basis of our analytic framework.

In the first place, the most recently formed cohorts should show the highest proportion of post-bourgeois responses and the lowest proportion of acquisitive responses, in every national sample. The respondents born after 1945 constitute the only group which (as far back as their memory reaches) has been socialized entirely under conditions of rising affluence, uninterrupted by major economic dislocations. As a first approximation, therefore, we would predict that: (1) the distribution of attitudes should resemble an L-shaped curve, with a very low proportion of post-bourgeois attitudes being found among respondents born before 1945, and a sharp rise in the prevalence of post-bourgeois values among those born after that date; conversely, the occurrence of acquisitive values should be uniformly high among all

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[16] There is a certain similarity between the configuration of "post-bourgeois" preferences and the well-known concept of "authoritarianism." Both concepts relate to the priorities one gives to liberty, as opposed to order. And—as we have just seen—the libertarian position seems linked with internationalism. This follows from the fact that, according to our analysis, the post-bourgeois groups have attained security in regard to both the safety and sustenance needs; insofar as the nation-state is seen as a bulwark protecting the individual against foreign threats, it is less important to post-bourgeois respondents. They have, moreover, a larger amount of "venture capital," psychically speaking, available to invest in projects having an intellectual and esthetic appeal—such as European unification. There are both theoretical and empirical differences between our position and that prevailing in the authoritarianism literature. We emphasize a process of historically-shaped causation which is not necessarily incompatible with, but certainly takes a different focus from, the psychodynamics of authoritarianism. Empirically, authoritarianism, like acquisitive value priorities, tends to be linked with lower economic status. By contrast, there are indications that children and youth tend to be *more* authoritarian than adults. (Stouffer, however, reported evidence of sizeable age-group differences among adult groups in degree of "Tolerance for Non-Conformity," with young adults far more tolerant than older adults; he sees the evidence as reflecting both life-cycle and intergenerational effects. See Samuel Stouffer, *Communism, Conformity and Civil Liberties* [New York: Doubleday, 1955], p. 89). In any event, neither previous explorations nor the present surveys revealed reasonably strong or consistent relationships between standardized F-scale items and the attitudes reported here. The two concepts seem related, but items which served as indicators of authoritarianism in earlier research appear to have limited applicability in the Europe of the 1970's. For a report of an earlier cross-national exploration of authoritarianism and internationalism, see Ronald Inglehart, "The New Europeans: Inward or Outward Looking?" *International Organization,* Vol. 24, No. 1 (Winter, 1970), pp. 129–139. The literature on authoritarianism is immense; the classic work is Theodor W. Adorno, *et al., The Authoritarian Personality* (New York: Harper, 1950); Cf. Richard Christie and Marie Jahoda, eds., *Studies in the Scope and Method of "The Authoritarian Personality"* (Glencoe: Free Press, 1954).

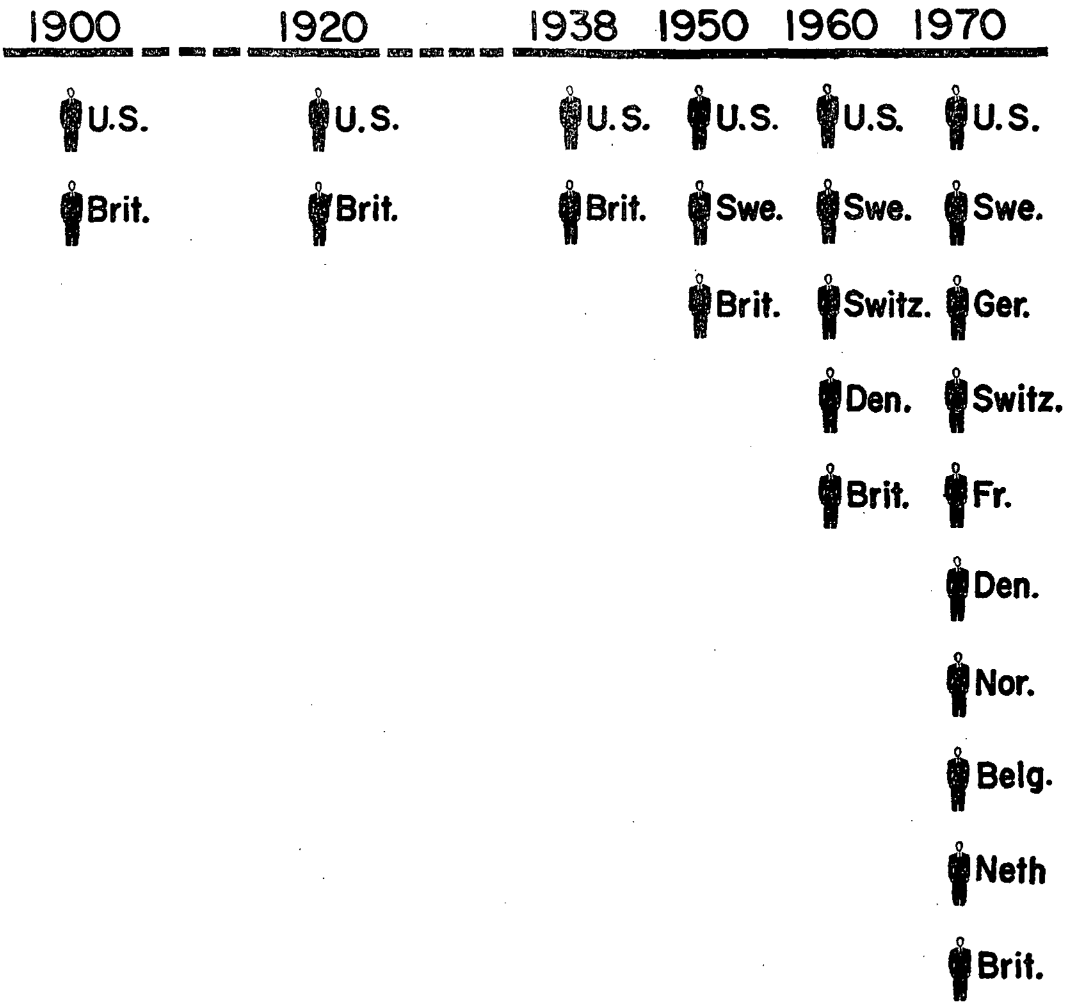

Figure 1. The decline in Britain's relative economic position. The U.S. and major European countries ranked according to per capita Gross National Product, 1900 to 1970. (Source: "The Economist," September 5, 1970, p. 69.)

cohorts born before 1945, with a precipitate drop as we reach the postwar cohorts. This pattern can only serve as a first approximation of course. It would be ridiculous to argue that no change in basic values can occur during adult life; our point is simply that the probability of such change becomes much lower after one reaches adulthood, and probably continues to decline thereafter. To the extent that adult relearning takes place, it would tend to smooth out the basic L-shaped curve. The fact that value preferences probably crystallize in different individuals at somewhat different ages, would also tend to have this effect.

We would not expect to find a zero incidence of post-bourgeois values even among the oldest cohorts: there has always been at least a small stratum of economically secure individuals, able to give top priority to nonacquisitive values. But this stratum should be smallest among the oldest cohorts if, indeed, it tends to reflect the level of affluence prevailing within a given society during a given cohort's pre-adult years.

By the same token the distribution of these value preferences should vary cross-nationally in a predictable fashion—reflecting the economic history of the given nation. Fortunately for our analysis, there are substantial differences in the 20th-century economic experiences of the nations in our sample. These variations enable us to make predictions about the relative *level* and *steepness* of the value-distribution curves for given nations. To put it briefly, high absolute *levels* of wealth in a given nation at a given time would predict relatively high proportions of post-bourgeois respondents among the cohorts socialized under those conditions; high *rates* of growth for a given country would predict relatively large *increases* in the proportion of post-bourgeois respondents, across that nation's age-groups. The economic progress of Great Britain, in particular, shows a sharp contrast with that of the other five nations. Throughout the first four decades of the 20th century, Britain—the home of the first Industrial Revolution—was by far the wealthiest country in Europe, and in world wide comparisons it ranked second only to the U.S. (and, sometimes, Canada) in per capita income. During the decade before World War II, among the nations in our sample, The Netherlands ranked closest to Britain (with a per capita income 71 per cent as high as the British) followed by France, Belgium and Germany, with Italy far behind (having only 27 per cent the per capita income of Britain). In the postwar era, the economically privileged position which Britain had long enjoyed began to deteriorate rapidly (see Figure 1). Although her absolute level of income rose gradually (interrupted by periods of stagnation), Britain was overtaken by one after another of her European neighbors—nearly all of which experienced much more rapid and continuous economic growth; these growth rates were particularly steep in the case of Germany and Italy (see Figure 2). By 1970, Britain had been outstripped by five of the six European Community countries, with the sixth (Italy) not far behind.

On the basis of these historical data, we can make four predictions about the expected value-distribution curves in addition to the L-shaped curve posited earlier in our first prediction. (2) Among those respondents who reached adulthood before World War II, the size of the stratum which had known economic security during its formative years would be small—but its *relative* size should be greater in the British sample than in the other national samples. Translated into expected survey results, this means that the British cohorts now in their mid-50's or older should show the highest frequency of post-bourgeois values. (3) The *rate* of value change found in Britain, however, should be much lower than that in the other five countries. Her economic growth rate since World War II has been approximately half that of the average among the European Community countries; as a first approximation, we might expect the rate of *increase* in post-bourgeois values found among Britain's younger cohorts to be half as great as that within the EEC. (4) In *prevalence* of post-bourgeois values among the younger cohorts, we might expect Britain to be outstripped by all of the European Community countries except Italy. (5) Among these six nations, Germany and Italy—the two countries experiencing the most rapid economic change during the post-war era—should show the greatest amount of intergenerational change in basic value priorities.[17]

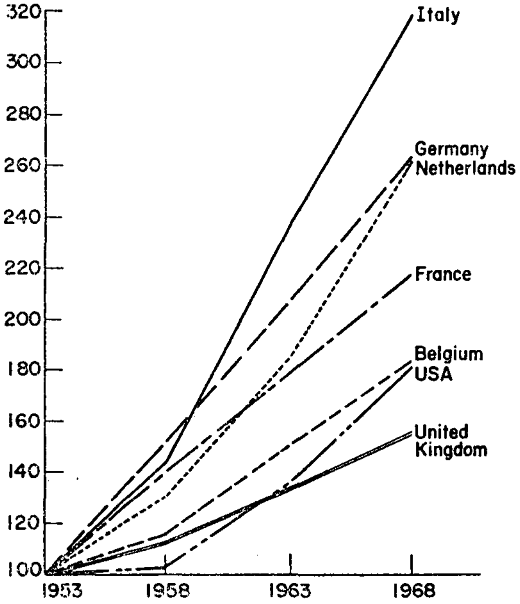

Figure 2. Economic growth, 1953–68. Based on indices of industrial production (1953 = 1.00). Source: "U.N. Statistical Yearbook, 1969."

The year 1953 is taken as our base line to avoid giving undue prominence to recovery from the devastation of World War II: using 1948 or 1945 as a base would tend to exaggerate the disparity between Germany and Italy (on one hand) and Great Britain (on the other hand).

With these five predictions in mind, let us examine the empirical relationship between value preferences and age cohort, within each national sample. (See Table 5.) Our basic prediction—that the younger cohorts will be less likely to show acquisitive value priorities and more likely to show post-bourgeois values—is confirmed strikingly. Among the oldest cohort, the disproportionate preference for the pure

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[17] It is difficult to interpret the cross-national pattern as a reaction to current events within the respective nations. There *is* considerable evidence of a recent law-and-order reaction in the face of student disorders in each of these countries. But if the cross-national differences were largely the result of such a reaction, we would expect to find the emphasis on order to be greatest in France (where the recent upheaval was greatest) and weakest in Britain (which has had the smallest amount of domestic disorder). The data manifestly fail to fit this pattern; we must explain them in terms of predispositions *anterior* to, rather than resulting from, the recent domestic disorders these countries have experienced.

Table 5. "Pure" Value Preferences, by Age Cohort

(Percentage choosing each pair)*

Age Range of cohort in 1970	Netherlands		Belgium		Italy		France		Germany		Britain	
	Acq.	P-B N	Acq.	P-B N	Acq.	P-B N	Acq.	P-B N	Acq.	P-B N	Acq.	P-B N
16-24	20%	29 (442)	19	26 (227)	18	28 (335)	21	20 (365)	21	23 (317)	25	14 (254)
25-34	27	16 (408)	35	13 (211)	30	15 (256)	35	11 (369)	35	15 (409)	29	9 (340)
35-44	36	14 (406)	28	19 (234)	36	11 (397)	36	14 (347)	46	8 (372)	29	8 (278)
45-54	29	15 (285)	29	13 (188)	37	8 (310)	39	10 (319)	47	7 (326)	37	5 (398)
55-64	37	7 (223)	37	8 (201)	42	7 (315)	48	6 (280)	60	4 (325)	41	8 (331)
65+	44	5 (138)	45	2 (235)	54	4 (193)	50	2 (366)	56	2 (265)	50	5 (374)
Difference between oldest & youngest cohorts:	48		50		60		47		56		34	
	−24	+24	−26	+24	−36	+24	−29	+18	−35	+21	−25	+ 9

* Number in parentheses is base on which percentages are calculated.

"acquisitive" pair is overwhelming: half or nearly half of the entire cohort choose that one pair, out of six possibilities. Most of the remaining respondents in this cohort are ambivalent; a relative handful—in no case more than five per cent—chooses the post-bourgeois set of priorities. Overall, acquisitive types outnumber post-bourgeois types by a ratio of better than 15:1 in this cohort. As we move up the table from the oldest to the youngest cohort, the proportion choosing the pure "acquisitive" pair falls off markedly, diminishing by considerably more than one-half in every sample except the British; even in the latter case, the decline is just equal to 50 per cent. As we move from oldest to youngest, the increase in the proportion choosing the post-bourgeois priorities is proportionately even greater: even in the British sample, where the indications of change over time are weakest, the post-bourgeois proportion nearly triples.

Moreover, we do find something resembling a modified L-shaped curve in the distribution of these responses: across the six national samples, by far the biggest discontinuity occurs as we move from the second-youngest to the youngest cohort. Even among the 25–34 year-old cohort, there is still a heavy plurality of acquisitive types over post-bourgeois types. A major shift occurs as we move to the one age cohort that has been socialized entirely in the postwar era:[18] the post-bourgeois group almost doubles in size (among the Continental samples), while the acquisitive group declines sharply. Within the youngest cohort, the post-bourgeois group has either reached approximate parity or moved ahead of the acquisitive group—except in Britain. Although value change occurs across the whole range of age cohorts, no transition is as sharp as the one associated with socialization in the postwar era.

Moving to cross-national comparisons, we note that our second prediction is also confirmed: Although the British sample as a whole has the *smallest* proportion of post-bourgeois types, among the cohorts who reached adulthood before World War II (those now more than 54 years of age), Britain shows the *highest* proportion of post-bourgeois respondents. She is very closely followed by the Dutch in this respect (the nationality which came closest to the British level of affluence in the prewar period).

Our third prediction also seems to be confirmed by the data: the rate of change across the British cohorts is much smaller than that found in any other country. The total number of points separating the oldest British cohort from the youngest is not much more than half as large as the range found in the German and Italian samples—where apparent intergenerational change is strongest (in keeping with our fifth prediction).

Our fourth prediction was that among the youngest cohorts, Britain should rank behind every country except Italy in her proportion of post-bourgeois respondents. This expectation is amply borne out: the British sample ranks far behind all the other samples—*including* the Italian, which seems to be a good deal more post-bourgeois than it should be on the basis of economic expectations. We will not attempt to provide an *ad hoc* explanation for this anomaly: It is puzzling, but on the whole the empirical findings seem to correspond to expectations drawn from economic history remarkably well.

Generational or Life-Cycle Interpretation?

At this point we should consider the possibility that the observed age-group differences reflect life-cycle factors, rather than intergenerational change. The large shift in value preferences which we find as we move from the second youngest to the youngest cohort is, indeed, what we would expect to find, on the basis of the conditions which governed the formative years of the respective cohorts. But the phenomenon might also be interpreted in life-cycle terms: the cutting point between the two age groups corresponds roughly to the age at which the average individual marries and starts a family. It could be argued, therefore, that the youngest cohort shows a tendency toward post-bourgeois values merely because these individuals are young and lack family responsibilities; when they get older, they will have the same value priorities as the older cohorts have now. Since responses to these items seem to be relatively well integrated into the individual's attitudinal structure—a fact which suggests attitudinal stability—such an interpretation seems rather unlikely. The finding that the age-cohort differences seem to reflect the economic history of the given nation makes the life-cycle interpretation still less satisfactory. And when we examine the data from still another perspective, any simple life-cycle interpretation becomes quite implausible.

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[18] Interestingly, this shift corresponds to the transition from the purportedly apolitical youth of the 1950's—the "Skeptical Generation" or "Uncommitted Youth," as they were called—to the relatively radical youth of the 1960's.

As we recall, my basic hypotheses predicted *two* sorts of effects associated with an ongoing transformation of value priorities. The first, which we have just examined, relates to age-cohort differences; the second relates to differing degrees of affluence. The hypotheses suggest that the degree of economic security an individual felt during his formative years may play a key role in shaping his later political behavior. For most of our sample, it is impossible (at this late date) to obtain a direct measure of this variable. We do have some indirect indicators, however. Perhaps the most accurate one is the respondents' level of formal education: in Western Europe (even more than in the U.S.) one's likelihood of obtaining a secondary or university education is very closely related to the socioeconomic status of one's family of origin. Insofar as it influences levels of education and career aspirations, the relative affluence of one's parents also tends to be correlated with the individual's own economic status. To the extent that this association holds, our data on the individual's *own* education, current occupation, and income should also serve as a rough indicator of the degree to which he was economically secure during his formative years. (Most of the women in our sample do not have independent occupations: for them, our indicators are their own education and the occupation of head of family.)

In terms of the indicators available to us, then, our prediction is that post-bourgeois values should be most prevalent among those who currently enjoy a relatively high socioeconomic status—although this indicator is understood to be important chiefly insofar as it reflects affluence during one's formative years. Let us test this hypothesis. Table 6 shows the distribution of value preferences according to socioeconomic status (ranked on the basis of a scale combining occupation and education). Table 6 summarizes the relationship between value priorities and socioeconomic status within the six national samples. As predicted, the lower socioeconomic groups are much more likely to select acquisitive value priorities than are the upper socioeconomic groups: overall, about 42 per cent of the lower socioeconomic category chooses the theoretically "pure" acquisitive value pair—more than double the proportion which makes that choice among the two highest socioeconomic categories. Conversely, the upper socioeconomic categories are much more likely to choose the post-bourgeois set of value priorities. Once again, Britain tends to be a deviant case: her social class differences (like her age-cohort differences) are smaller than those in the other countries.

On the whole, the relationship between age cohort and value priorities persists when we control for socioeconomic status (see Table 7). Despite the presence of some anomalies (especially in the Dutch sample), the predominant pattern is that the percentage choosing acquisi-

Table 6. Value Preferences by Socioeconomic Status

(Percentage choosing respective "pure" value pairs)

Socioeconomic Status*	Netherlands			Belgium			Italy			France			Germany			Britain		
	Acq.	P–B	N	Acq.	P–B	N	Acq.	P–B	N	Acq.	P–B	N	Acq.	P–B	N	Acq.	P–B	N
Lower S.E.S.	40%	7	(551)	38	6	(486)	38	10	(995)	47	4	(908)	49	7	(1319)	37	6	(1179)
Middle S.E.S.	29	20	(526)	33	15	(353)	30	14	(331)	35	11	(626)	38	11	(510)	40	8	(459)
Upper Middle S.E.S.	16	30	(365)	24	20	(86)	18	32	(105)	29	15	(369)	23	26	(139)	28	10	(261)
Upper S.E.S.	11	52	(66)	17	35	(95)	18	27	(135)	14	42	(143)	16	44	(44)	25	15	(73)

* "Upper" S.E.S. Group includes respondents from "Modern Middle" class backgrounds having university educations (see footnote 9 for our definition of the "Modern Middle Class"); "Upper Middle" S.E.S. includes members of that class, having a secondary level of education; "Middle" S.E.S. includes respondents from other occupational backgrounds (including traditional middle class) educated beyond the primary level.

tive priorities declines sharply, and the percentage choosing post-bourgeois priorities rises sharply, as we move from oldest to youngest cohorts. Perhaps the most significant aspect of Table 7 is the extent to which it tends to refute a life-cycle interpretation of the observed age-group differences; to uphold such an interpretation, we would have to posit the existence of totally different life cycles for working-class and middle-class respondents. To be sure, working-class youth tend to enter the work force and marry earlier than their middle-class peers—but in terms of value priorities, the two classes are out of phase not just by four or five years, but by nearly a generation. Within the youngest Dutch cohort, for example, the upper socioeconomic categories choose post-bourgeois priorities over acquisitive priorities by a ratio of 50:6, while 43 per cent of their lower socioeconomic peers choose acquisitive values—with *none* making post-bourgeois choices. In the Belgian sample, the corresponding ratios are 35:12 within the upper middle and upper socioeconomic categories, as contrasted with 14:24 within the lower socioeconomic category. On the basis of value priorities, a working-class Frenchman 20 years old corresponds to a middle-class Frenchman in his 50's. More or less the same thing can be said in regard to the other samples from the countries of the European Community.

The age-cohort variations shown in Table 7, then, can scarcely be explained as a result of the aging process alone. An explanation in terms of economic and physical security *during a formative period* accounts for the observed pattern of both age cohort and socioeconomic status differences in a parsimonious fashion. For this interpretation to be applicable, however, we must accept the hypothesis that these value priorities reflect an aspect of the individual's orientation which tends to persist over time.

Substantial age-cohort differences also persist when we apply finer controls for education by itself (see Table 8). Thus, although formal education seems to have a strong influence on the value priorities held by an individual, the age-cohort differences, are *not* simply due to the different levels of education characterizing given age cohorts (as shown in Table 1). Mul-

Table 7. Value Preferences by Age Cohort, Controlling for Socioeconomic Status

(Percentage choosing respective "pure" value pairs)

Britain

Age range of cohort in 1970	"Acquisitive" value preferences			"Post-Bourgeois" value preferences		
	Lower S.E.S. (N=1179)	Middle S.E.S. (N=459)	Upper-Mid. & Upper S.E.S. (N=334)	Lower S.E.S. (N=1179)	Middle S.E.S. (N=459)	Upper-Mid. & Upper S.E.S. (N=334)
16–24	26%	31	19	10%	19	16
25–34	24	35	34	10	7	8
35–44	25	43	29	7	7	10
45–54	38	43	25	4	8	4
55–64	44	40	33	6	8	14
65+	50	54	(34)*	5	3	(14)

Germany

Age range of cohort in 1970	"Acquisitive" value preferences			"Post-Bourgeois" value preferences		
	Lower S.E.S. (N=1319)	Middle S.E.S. (N=510)	Upper-Mid. & Upper S.E.S. (N=183)	Lower S.E.S. (N=1319)	Middle S.E.S. (N=510)	Upper-Mid. & Upper S.E.S. (N=183)
16–24	25%	20	10	15%	19	49
25–34	40	29	19	10	17	35
35–54	48	48	23	6	8	20
45–54	52	41	33	6	7	14
55–64	64	54	(32)*	3	6	(16)
65+	59	49	(33)	2	0	(11)

* Percentages based on fewer than 30 cases are enclosed in parentheses.

Table 7.—(*Continued*)

France

Age range of cohort in 1970	"Acquisitive" value preferences			"Post-Bourgeois" value preferences		
	Lower S.E.S. (N=908)	Middle S.E.S. (N=626)	Upper-Mid. & Upper S.E.S. (N=512)	Lower S.E.S. (N=908)	Middle S.E.S. (N=626)	Upper-Mid. & Upper S.E.S. (N=512)
16–24	30%	20	17	8%	18	31
25–34	41	41	26	2	6	23
35–44	45	36	24	6	17	17
45–54	49	26	32	4	10	24
55–64	53	40	36	5	9	8
65+	49	56	31	1	4	5

Italy

Age range of cohort in 1970	"Acquisitive" value preferences			"Post-Bourgeois" value preferences		
	Lower S.E.S. (N=995)	Middle S.E.S. (N=331)	Upper-Mid. & Upper S.E.S. (N=240)	Lower S.E.S. (N=995)	Middle S.E.S. (N=331)	Upper-Mid. & Upper S.E.S. (N=240)
16–24	23%	17	9	23%	25	40
25–34	31	26	13	11	7	37
35–44	36	30	25	9	15	23
45–54	38	39	24	8	7	19
55–64	46	44	(12)*	7	13	(5)
65+	53	(46)	(83)	4	(9)	(0)

Belgium

Age range of cohort in 1970	"Acquisitive" value preferences			"Post-Bourgeois" value preferences		
	Lower S.E.S. (N=486)	Middle S.E.S. (N=353)	Upper-Mid. & Upper S.E.S. (N=181)	Lower S.E.S. (N=486)	Middle S.E.S. (N=353)	Upper-Mid. & Upper S.E.S. (N=181)
16–24	24%	24	12	14%	20	35
25–34	35	38	30	4	12	30
35–44	32	33	20	11	16	36
45–54	37	23	20	10	19	9
55–64	42	34	26	3	18	18
65+	41	58	(50)*	3	0	(0)

Netherlands

Age range of cohort in 1970	"Acquisitive" value preferences			"Post-Bourgeois" value preferences		
	Lower S.E.S. (N=551)	Middle S.E.S. (N=526)	Upper-Mid. & Upper S.E.S. (N=431)	Lower S.E.S. (N=551)	Middle S.E.S. (N=526)	Upper-Mid. & Upper S.E.S. (N=431)
16–24	43%	23	6	0%	28	50
25–34	36	29	12	11	15	28
35–44	43	32	20	8	16	28
45–54	36	18	25	5	25	25
55–64	46	49	24	10	6	19
65+	40	(61)*	(13)	7	(0)	(13)

* Percentages based on fewer than 30 cases are enclosed in parentheses.

Table 8. Value Choices by Age Cohort, Controlling for Education

(Percentage choosing acquisitive or post-bourgeois pairs)

Age in 1970	Primary			Secondary			University		
	Acq.	P-B	N	Acq.	P-B	N	Acq.	P-B	N
Britain									
16–24	26%	12	(121)	25%	16	(73)	21%	19	(48)
25–34	27	9	(216)	34	5	(89)	21	21	(19)
35–44	28	6	(205)	33	12	(57)	38	13	(8)
45–54	40	5	(299)	30	6	(67)	18	0	(11)
55–64	42	6	(267)	40	9	(45)	33	22	(9)
65+	50	4	(315)	48	4	(23)	31	23	(13)
Spread, from youngest to oldest cohort:	+24	− 8		+23	−12		+10	+ 4	
Germany									
	25%	15	(235)	11%	36	(47)	7%	61	(28)
	36	12	(353)	28	31	(36)	18	47	(17)
	49	6	(330)	29	21	(28)	17	33	(6)
	50	7	(278)	31	3	(29)	18	27	(11)
	63	4	(278)	35	7	(29)	25	25	(8)
	59	2	(213)	44	0	(27)	20	20	(5)
Spread:	+34	−13		+33	−36		+13	−41	
France									
	30%	11	(84)	21%	17	(224)	6%	48	(52)
	39	2	(145)	40	9	(172)	6	48	(50)
	46	8	(170)	27	14	(139)	22	39	(36)
	45	4	(188)	32	17	(112)	15	39	(13)
	52	5	(184)	41	8	(74)	35	10	(20)
	49	1	(249)	55	3	(88)	35	5	(20)
Spread:	+19	−10		+34	−14		+29	−43	
Italy									
	23%	23	(199)	18%	25	(44)	7%	39	(87)
	33	9	(183)	29	21	(24)	14	40	(35)
	37	10	(326)	28	17	(18)	24	24	(41)
	38	7	(264)	43	14	(14)	15	15	(20)
	44	7	(278)	17	17	(12)	20	0	(15)
	53	4	(168)	60	20	(5)	75	0	(8)
Spread:	+30	−19		+42	− 5		+67	−39	
Belgium									
	26%	19	(27)	19%	23	(147)	14%	41	(44)
	41	3	(63)	31	14	(117)	32	36	(25)
	29	9	(98)	30	21	(106)	17	50	(24)
	33	8	(89)	28	21	(80)	8	0	(12)
	43	4	(126)	23	15	(60)	50	20	(10)
	43	3	(179)	52	0	(29)	40	0	(10)
Spread:	+17	−16		+33	−23		+26	−41	
Netherlands									
	35%	0	(40)	16%	36	(258)	0%	58	(19)
	35	10	(100)	19	19	(173)	10	52	(21)
	41	7	(122)	27	21	(146)	19	48	(21)
	32	8	(113)	23	24	(79)	11	56	(9)
	42	7	(82)	35	13	(48)	13	25	(9)
	42	6	(81)	52	0	(21)	33	17	(6)
Spread:	+ 7	+ 6		+36	−36		+33	−41	

tiple classification analyses[19] indicate that education is among the strongest predictors of value priorities. It ranks with occupation, religion, income levels, and age cohort as an important influence on basic values (although all four of the latter variables also seem to have substantial independent effects on value priorities, when we control for the effects of each of the other variables). Our own interpretation would emphasize that this is the case because education is our most accurate indicator of parental affluence during the respondent's formative years. It might very plausibly be argued, however, that this relatively strong relationship exists because of something based on education *itself*: for example, that under present circumstances, the process of formal education assimilates the individual into an elite political culture which stresses expressive values.[20] Indeed, we suspect that there may be some truth in the latter interpretation; but we regard it as a complementary rather than an alternative explanation. Our data do not contain a direct measure of economic security during one's formative years, so we cannot separate the two effects. But regardless of whether we regard the impact of education as being largely due to education *per se*, or a reflection of parental affluence, two important facts seem fairly clear: (1) the age-cohort differences are not due to educational differences alone—even the less educated members of the younger cohorts show a marked tendency to be less acquisitive and more post-bourgeois than the older cohorts (which may reflect the fact that in the postwar era, even the less educated have known relative affluence). (2) Even if the socioeconomic class differences are largely due to education *per se* rather than to affluence during formative years, we would expect them to persist over time: relatively high levels of formal education are a stable characteristic of the younger cohorts, which is not likely to disappear as the individuals age. In either case, we may be justified, therefore, in projecting changes over time as the younger (and more educated) cohorts replace the older groups in the adult electorate.

Table 9. Percentage Choosing "Freedom of Speech" by Age Group: Germany, 1962*

Age in 1962		
16–25	58%	not included in 1949 sample
25–30	52	
30–50	50	
50–65	40	
65+	34	

* Source: EMNID *Pressedienst* (Gallup-Institut, Bielefeld), cited in *Encounter*, Vol. 22, No. 4 (April, 1964), p. 53. Age groupings are those given in this source.

Ultimately, of course, our thesis can be proved or disproved only with the aid of longitudinal data—and, as we noted earlier, very little is available at present. A small body of relevant time-series data *is* available, however, and it seems worth examining. The EMNID institute of West Germany employed an item concerning value priorities in a series of surveys of German public opinion from 1949 through 1963; the question was, "Which of the Four Freedoms do you personally consider most important?" Like the items used to measure value priorities in our own survey, this was a forced-choice question, requiring the individual to make a selection among positively valued items, according to his personal priorities. And because the two leading choices by far were "Freedom from Want" and "Freedom of Speech," the choice an individual made probably tends to tap the dimension central to this inquiry—acquisitive versus post-bourgeois values. In 1962, for example, nearly half of the German sample ranked "Freedom of Speech" as the most important freedom. Let us look in Table 9 at the relationship between age and preference for that value in 1962 (unfortunately, the only year for which an age breakdown is available).

The pattern of age differences shown in Table 9 is similar to what we found in our own data: the young are much more likely to place a high priority on free speech than are the old. *Prima facie*, this age-relationship could be interpreted as reflecting either a life-cycle effect or intergenerational change.[21]

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[19] This analysis is similar to a multiple regression analysis, using dummy variables. For an explanation of the technique, see John A. Sonquist, *Multivariate Model Building: the Validation of a Search Strategy* (Ann Arbor: Institute for Social Research, 1970).

[20] Granting that this is the process at work, we must ask *why* this elite political culture gives relatively high priority to expressive values; one is tempted to draw on relative economic security to supply at least part of the answer. As is pointed out later in this section, however, higher education does not seem to be inherently linked with a libertarian political position; at other points in history, it has been associated with relatively authoritarian and conservative positions.

[21] Other possibilities also exist:

(1) It could be due to sampling error. We believe the latter possibility can be excluded, however: we have found a similar age-group pattern in all seven of the European surveys cited thus far; moreover, we have examined responses to items from a large number of American surveys which, implicitly or openly, ask the individual to choose between political liberties and

The former interpretation has a certain appeal: it is linked with the seemingly parsimonious assumption that nothing is really changing —young people will be like their parents when they get older. When examined a little more closely, it becomes apparent that the life-cycle interpretation is in no sense more parsimonious than the generational interpretation; indeed, it could be considered less so: though it assumes that the preferences of a nation as a whole will show no change, this result can be obtained only if each of the age-groups *within* the nation *does* change. Furthermore, it assumes—often without even considering the alternative—that the *direction* of any shift in preferences can be taken for granted: they must move in the direction which tends to erase the age-group differences. We agree it would be unrealistic to assume that individuals' value priorities will show *no* change over their adult lives—but it is conceivable that, as they age, they might move in the direction of giving a *higher* priority to libertarian values (for example), rather than a lower priority. Fortunately, we are able to examine trends in the percentages giving top priority to the item cited in Table 9. The EMNID institute's responses to the "Four Freedoms" item over the period 1949–1963 are reported in Table 10. The changes over time are impressive in size. These shifts might be attributed to two types of causes: 1) The mechanics of intergenerational change. This process has two aspects: (a) the recruitment of new (younger) members into the sampling universe from 1949 to 1963; and (b) mortality among members of the 1949 sample—most of the group aged 65+ in that year would have died off (its *youngest* members would be 79 in 1963). 2) Adult attitude change. The life-cycle effect constitutes a special case of adult attitude change, which assumes (in this case) that individuals will become less libertarian and more economically-motivated as they grow older.

Table 10. Changing Value Priorities: Germany, 1949–1963
"Which of the Four Freedoms do you personally consider most important?"
(Percentage choosing given item)*

	1949	1954	1958	1962	1963
Freedom from Want	35%	35	28	17	15
Freedom of Speech	26	32	44	47	56
Freedom from Fear	17	17	10	8	10
Freedom of Worship	12	16	16	13	14
N.A., D.K.	10	—	2	15	5

* Source: EMNID *Pressedienst*, cited in Table 9.

The data from Tables 9 and 10 enable us to estimate parameters for the two processes. While rough calculations indicate that only about one-third of the observed shift in value priorities from 1949 to 1962 might be attributed to the recruitment/mortality process, the *direction* of the remaining adult attitude change runs directly counter to that predicted by the life-cycle interpretation. It seems clear that, insofar as a shift in priorities occurred among individuals who were in the sampling universe in both 1949 and 1962, they tended to move in the "post-bourgeois" direction as they aged—not the reverse.

The time-series data reported in Table 10, moreover, has an excellent fit with recent German economic history. In the Germany of 1949, "Freedom from Want" was by far the leading choice. Germany's recovery from the devastation of World War II had just begun to get under way, and economic needs were extremely pressing for most of the population. Even under conditions of poverty, however, freedom of speech was the second-ranking choice. The fourteen years that followed were the years of the *Wirtschaftswunder*. Germany rose from poverty to plenty with almost incredible speed, and the two leading choices exchanged places: the percentage choosing "Freedom of Speech" more than doubled, while the percentage choosing "Freedom from Want" fell to less than half its former level (choice of the other two alternatives remaining relatively constant). These data suggest that a society may, indeed, show a shift in value priorities in response to changing conditions of scarcity. Admittedly, this must be regarded as an exceptional case: only rarely does so great a change in the average individual's economic situation

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threats to order or national security. A similar age-group pattern occurs in virtually all of them. See, for example, Hazel Gaudet, "The Polls: Freedom of Speech," *Public Opinion Quarterly*, 34 (Fall, 1970). The same pattern occurs in responses to comparable items in the S.R.C. 1968 presidential election survey. The likelihood of finding such a pattern in so many surveys from post-industrial societies as a result of sampling error appears negligible.

(2) The age-group pattern might be due to differential birth rates or life-expectancies among social groups having distinctive value priorities. These would tend to give the group having the higher birth rate (or shorter life expectancy) a disproportionately strong representation among the younger cohorts. Empirically, lower income groups tend to have had higher birth rates *and* shorter life expectancies than upper income groups over recent decades (For example, see Butler and Stokes, *op. cit.*, pp. 265–270). But lower income groups are relatively likely to express acquisitive value priorities. *Despite* this fact, post-bourgeois values are relatively widespread among the younger cohorts!

Table 11. The University Crisis: Value Climates in Student Milieu v. Administrative Milieu

(Percentage choosing the respective "pure" value pairs within the 16–24 year-old cohort ["students"] and the 45–54 year-old cohort ["administrators"] of the upper-middle/upper S.E.S. category)

	Britain		Germany		France		Italy		Belgium		Netherlands	
	Acq.	P-B	Acq.	P-B	Acq.	P-B	Acq.	P-B	Acq.	P-B	Acq.	P-B
Students' Milieu	19	16	10	49	17%	31	9	40	12	35	6%	50
Administrators' Milieu	25	4	33	14	32	24	24	19	20	9	25	25

occur within so short a space of time. But the direction of movement clearly conforms to the expectations generated by our hypotheses.

Some fragmentary but interesting time-series evidence from the other side of the Atlantic might be drawn from two excellent studies of the political consciousness of Yale students. Each seems to be the result of penetrating observation: Robert Lane's *Political Thinking and Consciousness;*[22] and Kenneth Keniston's *Young Radicals.*[23] The former study is based on material gathered in the 1950's and early 1960's; the latter study is based on observations made about ten years later. Being drawn from the same milieu with a decade's time-lag, they provide an impressionistic sort of time-series data. And the picture which emerges is one of profound change. Again and again in Lane's material one is made aware of the pressures toward conformity with a conservative norm: to be socially acceptable in the Yale of the late 50's, one felt obliged to identify with the Republican Party and to support the policies of the Establishment. The situation a decade later shows a fascinating contrast. As Keniston makes clear, the "Young Radicals" who had then become a salient part of the Yale scene were *not* acting out of youthful rebellion: they were advocating policies which seemed to them a more faithful implementation of the values that had been inculcated in their homes. Yet their views sharply conflicted with the social and foreign policies of the popularly elected governments, whether Democratic or Republican. In another book which was shaped by observation of Yale students, Charles Reich gives an insightful interpretation of this complex process of change.[24] His analysis, in part, is similar to our own: a younger generation has emerged which has a basically different perspective from earlier generations (Reich refers to the younger generation's value system as "Consciousness III"). My conclusions diverge from those of Reich chiefly in the extent to which I would generalize these changes. The present data suggest that although post-industrial societies may indeed be undergoing a transformation similar to the emergence of "Consciousness III," the process of transformation is decidedly uneven, and the earlier types of consciousness continue to be predominant even among youth—*except* in certain sectors: above all, the universities.

A life-cycle interpretation tends to write off such evidence of intergenerational differences as due to youthful rebelliousness or high spirits, often without considering the *type* of values motivating radical youth. Although I am not aware of a body of micro-analytic data from Europe comparable to the Yale studies just cited, observation of political activity on a gross level suggests a significant change in the values espoused by European student activists during the past generation or so. One need scarcely dwell on the Rightist and authoritarian aspects of student movements in Germany and Italy of the 1930's. What is perhaps less widely recognized is that the predominant thrust of political activism among *French* students in the 1930's also had a markedly conservative character: their most critical intervention in French politics undoubtedly took place in early 1934, when Monarchist and quasi-Fascist youth (mostly upper middle-class, and many of them from the universities) played a prominent role in a series of riots which very nearly overthrew the Third Republic.[25] Then, as now, British students seem to have been a deviant case: relatively liberal in the 1930's and relatively conservative in 1970.

The wave of intense student political activity which swept both Europe and North America in the late 1960's seems to have diminished today.[26] Was it a campus fad or does it represent a manifestation of broader changes in post-industrial society? I believe that the present data and analytic framework provide a useful perspective from which to interpret its implications.

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[22] (Chicago: Markham Publishing Company, 1969).

[23] (New York: Harcourt, Brace and World, 1968).

[24] *The Greening of America* (New York: Random House, 1970).

[25] See, for example, William L. Shirer, *The Collapse of the Third Republic* (New York: Simon & Schuster, 1969), pp. 201–223.

[26] Among the reasons for this decline in activity, the fact that some concessions were made to some of

To illustrate, let us look at Table 11 (which is simply a subset of Table 7). University students in these countries tend to be drawn overwhelmingly from the upper and upper-middle socioeconomic strata. If we take the youngest cohort of these strata as roughly indicative of the value climate in the student milieu in each country—and if we regard the 45–54 year-old cohort of the same socioeconomic category as indicative of the milieu from which the university administrators are drawn—we can form an idea of the contrasting value climates within the two milieux.[27] Our data suggest that there have always been a certain number of people with the value priorities which we call post-bourgeois, but that until recently they were a relatively small minority. Within the last decade they seem to have become relatively numerous—constituting a major political bloc in themselves; furthermore, they tend to be brought together as a group capable of setting the prevailing tone in an important institutional context—the universities. As Table 11 indicates, post-bourgeois types now seem to hold a heavy plurality over the traditionally predominant acquisitive types in the student milieu of five of our six national samples. While they may not yet constitute an absolute majority even in this setting, their preponderance over the acquisitive types may enable the post-bourgeois group to act as the leading influence on many of their "ambivalent" peers. By contrast with the student milieu, the value climate from which the administrators are drawn tends to contain a plurality of acquisitive over post-bourgeois types. The administrators, moreover, are subject to relatively strong pressures from society as a whole—which tends to be far more conservative in its value priorities than are the administrators themselves. The result (rather frequently) is not simply disagreement, but conflicts which seem unamenable to compromise—because they are based on fundamentally different value priorities. (An incidental outcome seems to be the frequent rotation of university administrators.)

A notable exception to the foregoing pattern appears in the British sample, where there still seems to be a narrow plurality choosing acquisitive value priorities, even within the student milieu—a finding which may go far to explain the relative tranquility of the university scene in that country. While there have been a few relatively subdued uprisings at British universities in recent years, one can point to student explosions which dwarf them in every one of the five other countries.

According to our data, West Germany seems to be the country which has the greatest degree of intergenerational strain in her universities, with a 3:1 predominance of acquisitive values in the "administrative" milieu and a 5:1 predominance of post-bourgeois value choices in the "student" milieu. This may seem momentarily surprising, since France is clearly the country in which the most resounding student uprising to date has taken place. To be sure, our data indicate considerable intergenerational strain in France, as well, but it seems to be less extreme than in the German case. These facts serve to remind us that survey data cannot be interpreted without reference to the institutional and geographical context from which they are drawn. We would attribute the differing outcomes to structural factors: important manifestations of student discontent took place at a number of locations in Germany well before they occurred in France. But the high degree of educational and administrative centralization in France meant that when an explosion *did* take place in Paris, it was a crisis that engulfed the whole country.

The hypothesis of intergenerational change

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the student demands is probably the most obvious factor, but I suspect that its importance is overrated. Another reason is that major political confrontations along the acquisitive/post-bourgeois dimension are likely to be counterproductive for the latter group under current conditions: the acquisitives still seem to hold a heavy numerical predominance—as became increasingly apparent on both sides of the Atlantic by the end of the 1960's. Still another factor seems pertinent in America: the economic recession of 1970 may have drawn greater attention to economic considerations on the part of groups which had previously given them little notice. The conventional wisdom holds that economic troubles tend to help the traditional Left; paradoxically (but in keeping with our analysis of intergenerational change) we would expect them to tend to *undermine* the New Left.

[27] Except among the youngest cohort, we do not have a large enough number of university-educated respondents to permit reliable estimates of the responses of those who actually *have* university educations. Within the youngest cohort, we do have at least 30 student respondents from four of our six countries; they tend to be somewhat more post-bourgeois than other members of their age group and socioeconomic stratum, but only moderately so: they are, on the average, four percentage points less acquisitive and seven points more post-bourgeois than their peers in Table 11. This suggests that it is not principally the university milieu which accounts for their value priorities (although this seems to play a part) but the fact that the students are from the youngest and most affluent social categories.

in value priorities (based on different levels of economic scarcity during a cohort's formative years) seems to have a good fit with a wide variety of evidence: with the attitudinal patterns of the respective age cohorts, and with those of given socioeconomic strata in samples from six nations; with the economic history of given nations and with cross-national differences in economic experience; and with what time-series data are available. It would be foolish to deny that individuals can and do change during their adult years. But if one's malleability is relatively great during preadult years and tends to decline thereafter, we would expect to find residues from formative experiences in the response patterns of the various adult cohorts.[28] Weighing the evidence as a whole, it seems to me that our data do give a rather strong suggestion of intergenerational change.

Value Priorities and Political Partisanship

The patterns of value preferences outlined above may represent a potential force for long-term political change. They might encourage the development of new political parties, relatively responsive to emerging value cleavages. Or they might lead to a realignment of the social bases of existing political parties, making age an increasingly important basis of cleavage (during a transitional period) and eventually, perhaps, tending to reverse the traditional alignment of the working class with the Left, and the middle class with the Right. For, in terms of the value priorities discussed in this article, upper status respondents are far likelier than lower status respondents to support a set of post-bourgeois principles which seem more compatible with parties of movement than with parties of order. Do we find any relationship between political party choice and our indicators of underlying value preferences? The respondents in each of our samples were asked:

> "If there were a General Election tomorrow, for which party would you be most likely to vote?"

Responses to this question are cross-tabulated with the two "pure" value pairs in Table 12; the parties are ordered according to the conventional notion of a Left-Right continuum.

In the British sample, the differences we find are of moderate size, but they are in the expected direction: respondents choosing post-bourgeois values are more likely to support the Labour Party than are acquisitive-type respondents; the intergroup differences amounts to eight percentage points. The post-bourgeois group is also relatively likely to support the Liberal Party, and the relative gains for both other parties come at the expense of the Conservatives—who are supported by a solid majority of the acquisitives, but by a minority of the post-bourgeois group. A somewhat similar pattern appears in the Belgian data.

In all four of the other countries we find quite sizeable differences in the partisan preferences of the two groups, and the differences are consistently in the expected direction: within the Dutch sample, for example, post-bourgeois respondents are more likely to support the parties of the Left by a margin of 23 percentage points; they give heavier support to the parties traditionally considered to be of the Left by a spread of 26 points in Italy; and by a spread of 15 points in Germany (22 points if we view today's F.D.P. as a party of the Left, which in some respects seems to be the case).

In France, the differences are the most impressive of all: post-bourgeois respondents are more likely to support parties usually considered Leftist by a margin of 36 percentage points over the acquisitives. A solid majority (56 per cent) of the latter group supports the Gaullist U.D.R. and their allies, the R.I.; while, by contrast, a bare 16 per cent of the post-bourgeois group supports the Gaullist coalition! Although it enjoys a wide plurality in the nation as a whole, the Gaullist coalition draws an almost insignificant minority of support from the group holding post-bourgeois value priorities. This finding tends to confirm our interpretation of the May Revolt mentioned earlier—that France's crisis of 1968 brought about a partial repolarization of the electorate according to underlying value preferences (with many working-class respondents shifting to the Gaul-

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[28] In their analysis of British panel survey data gathered in 1963, 1964 and 1965, Butler and Stokes, *op. cit.*, pp. 58–59, comment:

> A theory of political 'senescence' as it is sometimes called, fits comfortably the more general belief that the attitudes of youth are naturally liberal or radical, while those of age are conservative. . . . In the 1960's Conservative strength tended to be weakest among those born in the 1920's and just before. Electors younger than this tended actually to be a little *more* Conservative than those who lay within the precincts of early middle age. This irregularity, although an embarrassment to any simple theory of conservatism increasing with age, can readily be reconciled with the concept that the *conservation* of established political tendencies is what increases with age . . . we must ask not how old the elector is but when it was that he was young.

For an excellent example of age-cohort analysis based on data at the elite level, see Robert D. Putnam, "Studying Elite Political Culture: the Case of 'Ideology,'" *American Political Science Review*, 65 (September, 1971). Putnam finds evidence of significant intergenerational changes in basic political style among British and Italian politicians.

Table 12. Political Party Choice by Value Preferences

(Percentage choosing given political party)

Value Pref:	Britain				Germany				
	Labour	Liberal	Conserv.	N	SPD	FDP	CDU/CSU	NPD	N
Acquisitive	37%	7	57	(570)	48%	5	46	2	(648)
Post-Bourg.	45	9	46	(126)	63	12	23	2	(164)
Difference:	+8	+2	−11		+15	+7	−23		
	France				Italy				
	Left	Center	UDR, RI	N	Left	DC, PRI	Liberal	Extreme Right	N
Acquisitive	34%	10	56	(533)	28%	56	8	9	(398)
Post-Bourg.	70	15	16	(170)	54	38	8	1	(168)
Difference:	+36	+5	−40		+26	−18		−8	
	Belgium				Netherlands				
	Socialist	Liberal (PLP)	Christian Social	N	Socialist, Dem. '66		Liberal	Confessional	N
Acquisitive	31%	13	56	(253)	46%		12	43	(315)
Post-Bourg.	38	26	37	(117)	69		14	17	(216)
Difference:	+7	+13	−19		+23		+2	−26	

list side, while post-bourgeois elements from the middle class shifted to the Left). This sudden shift in vote from 1967 to 1968 does not seem to have been simply a temporary reaction to the 1968 crisis, with the voters returning to their normal partisan allegiance after the emergency had faded away. On the contrary, the French electorate still seems to retain an unequalled degree of political polarization according to value preferences in 1970, nearly two years after the May Revolt. This interpretation tends to be supported by data from a panel survey reported elsewhere.[29] The apparently enduring nature of this redistribution of political positions, once it has taken place, suggests that it may, indeed, correspond to relatively deep-seated values. In this connection, it seems significant that the other two countries in our survey which have experienced the most massive New Left upheavals (Germany and Italy) also show relatively high degrees of polarization according to value priorities, although its magnitude remains smaller than what we find in France. By contrast, Great Britain (apart from ethnic conflicts in Northern Ireland) has probably had the greatest measure of domestic tranquility among these countries in recent years—and shows a relatively weak relationship between value priorities and political party choice.

Admittedly, we have not mapped out in any precise fashion the differences between the political goals of the acquisitive and post-bourgeois groups: the latter group may still be in the process of defining a program. Moreover, there is at least an equal lack of precision in the party labels which we have just employed: we regard "Left" and "Right" as merely convenient shorthand terms under which to group (for cross-national comparisons) two sets of parties which tend to differ in being relatively conservative or relatively change-oriented, but which otherwise vary a good deal from country to country. To be sure, the acquisitive and post-bourgeois types of respondents *do* seem to react quite differently to these two sets of parties, and the pattern is fairly consistent cross-nationally. But the cleavage is *not* one which runs neatly along the traditional Left-Right dimension. Perhaps for this reason political polarization according to underlying value preferences seems much more pronounced in relation to what might be called New Left parties (in countries where they exist) than in relation to what might be called the Traditional Left. To illustrate, let us take a closer look at the vote for certain small parties which seem to have a distinctive appeal for the post-bourgeois constituency (see Table 13).

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[29] Philip Converse and Roy Pierce noted a sizeable shift to the Right from 1967 to 1968, within a panel of respondents asked to rank themselves on a Left-Right continuum in both years. After re-interviewing these respondent a third time, they report that more than 99 per cent of the change from 1967 to 1968 was preserved in 1969. See Converse and Pierce, "Basic Cleavages in French Politics and the Disorders of May and June, 1968," paper presented at the 7th World Congress of Sociology, Varna, Bulgaria, September, 1970.

In the French case, the PSU emerged from the crisis of May and June, 1968, as the political embodiment of the New Left, the only significant party which had unambiguously endorsed the May Revolt. Although it polled only 4 per cent of the vote nationally, and is supported by only 2 per cent of the acquisitives in our sample, it draws far more than this share of support among the post-bourgeois constituency—getting fully 29 per cent of this group's preferences. By comparison, the other parties of the French Left enjoy only a relatively small advantage among the post-bourgeois group—getting 9 percentage points more support there than among the acquisitive constituency. A similar pattern applies to support for two other parties which might be said to have a more or less New Left coloring: *Demokraten '66* in The Netherlands and the P.S.I.U.P. in Italy. The post-bourgeois group shows a marked preference for these parties, over the other parties conventionally regarded as of the Left.[30]

When we turn to the Belgian case, we find a rather surprising phenomenon. In traditional terms, we probably would not view the Belgian separatist parties as characteristically of the Left at all. But in their basis of recruitment, these parties (both Flemish and Walloon, but predominately the former) play a role comparable to that played by the PSU in France: they draw their strength very disproportionately from the post-bourgeois types, rather than from the acquisitives. In France, the ratio is nearly 15:1; in Belgium there is nearly a 4:1 over-representation of post-bourgeois as compared with acquisitive types. Indeed, when we include the separatist parties in our analysis, the Belgian Socialists actually show a slight *deficit* among the post-bourgeois group, when compared with the acquisitives (in Table 13).

The "New Left" parties and the Belgian separatists might seem to have little in common, other than a radical opposition to fundamental aspects of the established social system. But this disparity of political goals, juxtaposed with an apparent similarity in social bases and underlying value preferences, leads us back to a suggestion about the nature of post-bourgeois politics which was mentioned earlier: an important latent function may be to satisfy the need for belongingness. According to Maslow, this need comes next on the individual-level hierarchy, after needs related to sustenance and safety have been fulfilled. I would acknowledge and emphasize the importance of the manifest goals of a given movement in a given context; but it

Table 13. Political Party Choice by Value Preferences: Effect of the New Left and Belgian Separatist Parties

(Percentage choosing given political party)

Value Pref:	France: PSU	France: Other Left	France: Center	France: UDR, RI	France: N	Italy: PSIUP	Italy: Other Left	Italy: DC, PRI	Italy: Liberal	Italy: Extreme Right	Italy: N
Acquisitive	2%	32	10	56	(533)	1%	26	56	8	9	(398)
Post-Bourg.	29	41	15	16	(170)	7	47	38	8	1	(168)
Difference:	+27	+ 9	+ 5	−40		+6	+21	−18		−8	

Value Pref:	Netherlands: Dem. '66	Netherlands: Socialist	Netherlands: Lib.	Netherlands: Confessional	Netherlands: N	Belgium: Separatist	Belgium: Socialist	Belgium: Liberal	Belgium: Christian Social	Belgium: N
Acquisitive	13%	32	11	43	(315)	10%	28	12	50	(271)
Post-Bourg.	38	31	14	17	(216)	36	24	16	23	(128)
Difference:	+25	− 1	+ 3	−26		+26	− 4	+ 4	−27	

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[30] In the Italian case, however, the Communist party *also* seems to enjoy a relative preference within the post-bourgeois constituency: the PCI and PSIUP combined are supported by seven per cent of the acquisitives and by 30 per cent of the post-bourgeois group (leaving the two Socialist parties only a slightly greater proportion of support from the post-bourgeois group than from the acquisitives). It appears, then, that members of our Italian sample react to the PCI almost as if it were a New Left party—an interesting finding, in view of the fact that support for the French Communist party does *not* show a similar pattern; one wonders if the PCF cut itself off from post-bourgeois support in repudiating the May Revolt.

also seems likely that protest movements which are in radical conflict with their environment provide their members with a sense of belongingness. In the midst of large, anonymous, bureaucratically-organized societies, these movements may become tight little communities which are bound together all the more closely because they have a sense of radical opposition to, and isolation from, the surrounding society. Insofar as the drive for belongingness is an important component of these movements, their ideological content can be quite flexible. If we view the underlying dimension as based in part on this motivation, there is common ground between the Belgian Separatists and the New Left groups.

The similarity goes beyond this. The Flemish separatists clearly are not seeking economic gains. Indeed, they seem prepared to sacrifice them for what they regard as cultural and humanitarian gains. In this respect also, they might be grouped with the New Left. After the need for belongingness, the next priorities (according to Maslow) are for self-esteem and self-actualization, and for fulfillment of one's intellectual and esthetic potential. In a somewhat chaotic way, most of these (postacquisitive) values seem to be reflected in the issues espoused by the New Left: the movement reflects a broad shift in emphasis from economic issues to life-style issues.[31]

We find a quite interesting relationship between value priorities and political party choice in our data. I have spoken of this phenomenon as reflecting a tendency toward reordering political party choices to bring them into harmony with underlying values. But this line of reasoning assumes a causal relationship, in which the value preference is an independent variable capable of influencing current party choice. To what extent is this assumption justified? It could be argued that the association between value priorities and party choice is spurious—that it results from the fact that given individuals have been raised in relatively conservative (or relatively Left-oriented) backgrounds, shaping them in a way which accounts for the presence of *both* the value preferences and the political party choice currently expressed.

It is difficult to provide a conclusive demonstration of what caused what, but we can subject the foregoing interpretation to an interesting test. Our respondents were asked a series of questions to ascertain what had been their parents' political party preference or (failing this) their general Left-Right *tendance*. Let us examine the relationship between value priorities and current party choice, controlling for the political background in which the respondent was raised (see Table 14). A comparison of the N's given for each group in Table 14 indicates that there is, indeed, some tendency for the children of Left-affiliated parents to show a relative preference for post-bourgeois values; the strength of this tendency varies considerably from country to country. But for present purposes, the crucial finding which emerges from Table 14 is that, even when we control for this source of variation, quite substantial differences persist between the political party preferences of acquisitive-oriented respondents and those of post-bourgeois respondents. In many cases, these differences become even *larger* than they were in Table 12. Table 14 shows the flow of voters from the party in which they were raised, to other parties—and the flow certainly does seem to be influenced by the value priorities of the individual. In the British sample, evidence of intergenerational defection from the two major parties is relatively weak, and we find two mildly anomalous cases (in which post-bourgeois respondents are a trifle less likely to support the Labour Party than are the acquisitive respondents). Even in the British sample, however, the net tendency is for Labour to be relatively strong and the Conservatives relatively weak among the post-bourgeois group, holding parental background constant. In our Dutch sample, among those raised by parents who supported one of the confessional parties, 78 per cent of the group showing acquisitive values remain faithful to those parties; by contrast, among those indicating post-bourgeois values only 44 per cent have stayed with the church-linked parties—while an equal number have shifted their support to the parties of the Left (the Socialists; or *Demokraten* '66). Among Dutch respondents who were raised by supporters of the Socialist party, there seems to be greater continuity; fully 92 per cent of the post-bourgeois group say that they, too, would vote for the Left; among the acquisitive-oriented group, however, we find a rate of defection which is twice this high.

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[31] This ordering of priorities is, of course, not new in itself. Weber and Veblen, among others, called attention to the disdain for economic striving and an emphasis on distinctive life styles among economically secure strata throughout history. Veblen interprets the anti-acquisitive life style of past leisure classes as an attempt to protect their superior status by excluding individuals rising from lower economic levels. See Thorstein Veblen, *The Theory of the Leisure Class* (New York: Modern Library, 1934). It is highly dubious whether this interpretation applies to the contemporary post-bourgeois group as a whole. Its members appear universalistic in outlook and sometimes seem to imitate the life-style of *lower* strata. Conspicuous consumption seems to play a relatively small role in their behavior—unless we interpret going barefoot as a devious variation on conspicuous consumption. We would view needs for intellectual and esthetic self-realization as political motivations *in themselves.* Concern for pollution of the environment and the despoiling of its natural beauty—issues which played a minor political role until quite recently—have suddenly become prominent, with the emergence into political relevance of the current youth cohorts. These concerns may be justified in terms of self-preservation ("We are about to suffocate beneath an avalanche of garbage") but this argument may be somewhat hyperbolic: I suspect that behind this new wave of protest, there may be a heightened sensitivity to the esthetic defects of industrial society. It seems clear that other factors are *also* involved in the emergence of a New Left: situational factors unique to a given movement in a given society. I will not attempt to deal with them in this cross-national analysis.

Quite sizeable differences appear in the Italian sample; most notably, among those raised in a Christian Democratic or Centrist background, only 4 per cent of the acquisitive-type respondents defect to the Left—as compared with 33 per cent among the post-bourgeois respondents. In the German sample, somewhat similarly, post-bourgeois respondents from Christian Democratic backgrounds show a relatively strong tendency to defect from this political affiliation: while 63 per cent of the "acquisitive" respondents remain in the Christian Democratic fold, only 46 per cent of the post-bourgeois respondents do so. The partisan shift seems to reflect a relative drawing away from the church-linked parties on the part of the post-bourgeois group:[32] it continues the trend toward secularism traditionally associated with the Left. Indeed, the post-bourgeois group seems noticeably more sensitive to the supposedly outworn religious/secular cleavage than to the socioeconomic one: consistently, across our samples, the Christian Democratic parties show a heavy relative loss among this constituency, while the Liberal parties—which emphasize freedom of expression but often are more conservative on socioeconomic issues than the Christian Democrats—show a relative gain. The shift, indeed, seems more responsive to life-style values than to economic ones.

The most dramatic evidence of intergenerational change in political party loyalties is found in the French sample. Among the group raised within families which supported political parties of the Right, those with acquisitive value priorities are very likely to continue in that tradition: 91 per cent support the Gaullist coalition. There seems to be an astoundingly high rate of defection among the post-bourgeois group, however; 70 per cent of them indicate that they would vote for one of the parties of the Left! Conversely, among those raised in a family which preferred the Left, there is little defection to the Gaullist coalition. Among the acquisitive value group, the rate of defection to the Gaullists is nearly five times as high: a substantial 29 per cent say that they would vote for one of the governing parties.

A number of the cells in Table 14 contain too few cases to be significant by themselves,[33] but the overall pattern is clear: the presence of post-bourgeois values is linked consistently with a relative tendency to remain loyal to the Left, among those who were brought up in that tradition, and with a tendency to shift to the Left among those who were raised in other political climates. Jennings and Niemi have found evidence that recall data (such as ours) tends to exaggerate the degree of consistency between political party preferences of parent and child (perhaps as a result of the respondent's tendency to reduce cognitive dissonance).[34] This finding implies that, if anything, our data probably *understate* the degree to which intergenerational party shift is taking place.

Implications of Intergenerational Change

Our conclusion, then, is that the transformation of value priorities which our data seem to indicate *does* imply a change in the social basis of political partisanship in most, if not all, of these countries. This change may already have been under way for some time. To illustrate: In the first elections of the Fifth Republic, the French electorate apparently voted along class lines to a very considerable extent. Lipset, for example, provides a table showing that working-class voters were 29 per cent more likely to support the parties of the Left than were members of the modern middle class, in 1958.[35] Our

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[32] The linkage between church and party is most explicit on the Continent, but the British Conservative Party is no exception to this pattern: affiliation with the Established Church of England is strongly linked with preference for the Conservative Party. Even when we control for social class, the Anglicans in our sample are more likely to favor the Conservative Party than are members of minority faiths or non-religious respondents, by a margin of nearly 20 percentage points. The more frequently one attends the Anglican Church, moveover, the more likely one is to support the Conservatives.

[33] The reduced number of cases is due to the fact that here we are dealing only with those respondents:

1. Who have a political party preference—which they are willing to disclose; *and*
2. Whose *parents* had a political party preference—which was *known* by the respondent.

[34] See M. Kent Jennings and Richard G. Niemi, "The Transmission of Political Values from Parent to Child," *The American Political Science Review,* 62 (March, 1968), pp. 169–184.

[35] Calculated from Seymour M. Lipset, *op. cit.,* Chapter V, Table IV. Our comparison focuses on the two more dynamic groups of industrial society—the workers, on one hand, and the modern middle class on the other hand. Although the principle is similar, our measure of class voting, therefore, is not identical

Table 14. Intergenerational Party Shifts: Political Party Choice by Value Preferences, Controlling for Parents' Political Party

(Percentage choosing given party)

Britain

Value Pref:	Parents preferred Labour				Parents preferred Liberals				Parents preferred Conservatives			
Respondent would vote:	Lab.	Lib.	Cons.	N	Lab.	Lib.	Cons.	N	Lab.	Lib.	Cons.	N
Acquisitive	64%	5	31	(185)	23%	17	61	(64)	12%	3	85	(171)
Post-Bourg.	72	7	21	(47)	22	34	44	(9)	10	13	77	(31)
Difference:	+ 8	+2	−10		− 1	+17	−17		− 2	+10	− 8	

Germany

Value Pref:	Parents preferred Socialists				Parents preferred Liberals, FDP				Parents preferred Christian Democrats			
Respondent would vote:	Soc.	FDP	Chr. Dems.	N	Soc.	FDP	Chr. Dems.	N	Soc	FDP	Chr. Dems.	N
Acquisitive	87%	4	9	(78)	13%	63	13	(8)	35%	2	63	(115)
Post-Bourg.	87	8	6	(36)	40	60	0	(5)	45	7	46	(41)
Difference:		+4	−3						+10	+5	−17	

France

Value Pref:	Parents preferred "Left," Comm., Soc.					Parents preferred Center, MRP					Parents preferred "Right," Indep., Gaullist				
Respondent would vote:	PSU	Other Left	Center	UDR, RI	N	PSU	Other Left	Center	UDR, RI	N	PSU	Other Left	Center	UDR, RI	N
Acquisitive	6%	60	5	29	(106)	0%	8	69	23	(13)	0%	4	5	91	(118)
Post-Bourg.	25	52	8	6	(52)	35	26	39	0	(23)	35	35	0	29	(34)
Difference:	+19	− 8	+3	−23		+35	+18	−30	−23		+35	+31	−5	−62	

Italy

Value Pref:	Parents preferred "Left," Com., Soc.					Parents preferred "Center," Chr. Dems.					Parents preferred Liberals, Extr. Right				
Respondent would vote:	Left	DC, PRI	Lib.	Extr. Right	N	Left	DC, PRI	Lib.	Extr. Right	N	Left	DC, PRI	Lib.	Extr. Right	N
Acquisitive	81%	13	4	2	(53)	4%	92	2	3	(119)	33%	7	60	0	(15)
Post-Bourg.	92	5	3	0	(38)	33	64	4	0	(55)	75	0	25	0	(8)
Difference:	+11	− 8	−1	−2		+29	−28	+2	−3		+42	−7	−35		

Belgium

Value Pref:	Parents preferred Socialists					Parents preferred Catholic *tendance*					Parents preferred Liberals				
Respondent would vote:	Sep.	Soc.	Liberal (PLP)	Chr. Soc.	N	Sep.	Soc.	Liberal	Chr. Soc.	N	Sep.	Soc.	Liberal	Chr. Soc.	N
Acquisitive	—	83%	10	8	(40)	9	10	5	76	(101)	—	18	59	24	(17)
Post-Bourg.	11	78	11	—	(18)	38	3	11	49	(37)	40	—	60	—	(10)
Difference:	+11	− 5	+ 1	− 8		+29	−7	+ 6	−27		+40	−18	+ 1	−24	

Netherlands

Value Pref:	Parents preferred Socialists				Parents preferred confessional party (KVP, ARP, CHU)				Parents preferred Liberals			
Respondent would vote:	Soc., D'66	Liberal	KVP, ARP, CHU	N	Soc., D'66	Liberal	KVP, ARP, CHU	N	Soc., D'66	Liberal	KVP, ARP, CHU	N
Acquisitive	86%	4	11	(57)	16%	6	79	(102)	23%	73	5	(22)
Post-Bourg.	92	8	0	(73)	44	11	44	(72)	41	59	0	(18)
Difference:	+ 6	+4	−11		+28	+ 5	−35		+18	−14	−5	

1968 survey indicated that the percentage spread between social classes was only about half this size in 1967 and that it dropped several points from 1967 to 1968. Our 1970 data indicate little tendency for the French electorate to return to the 1967 level of class voting.

Paul Abramson, moreover, has recently reported evidence of a decline in the social class basis of political partisanship in France, Germany, and Italy—although not in Great Britain.[36] Our own data suggest a pressure that should tend to reduce the incidence of class voting in Britain, but this pressure seems to be a good deal weaker there than in the Continental countries. We would expect the extent to which partisan repolarization actually takes place to be limited by the relative strength of existing political party identification in given countries: the comparatively high degree of repolarization apparent in France may have been facilitated by the relatively weak sense of political party identification which characterized the electorate of that country until very recently. Conversely, the relatively small amount of repolarization indicated in our British sample may reflect the presence of comparatively strong political party loyalties in Britain. A recent analysis of socialization data by Jack Dennis and Donald McCrone, for example, suggests that feelings of identification with a political party were less widespread and less intense in France than in any of five other Western democracies studied (although Dennis and McCrone find evidence of an increase over time in political party identifiers in France, a finding which our own data support). According to Dennis and McCrone, the publics of Great Britain and the U.S. apparently rank highest in extent and intensity of political party identification, with Germany and Italy ranking at intermediate levels.[37]

There may be still another reason why Britain continues to maintain the traditional class-vot-

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with that used by Robert R. Alford in *Party and Society* (Chicago: Rand McNally, 1962). The traditional middle class, as a stagnant or declining element in the economy, has not shown a change comparable to that which apparently has taken place among the modern middle class; combining these two groups (as Alford does) dampens the effect we are describing.

[36] See Paul R. Abramson, "The Changing Role of Social Class in Western European Politics," *Comparative Political Studies* (July, 1971). Seymour M. Lipset and Stein Rokkan argue that "the party systems of the 1960's reflect, with but few significant exceptions, the cleavage structures of the 1920's"; see Lipset and Rokkan, *Party Systems and Voter Alignments: Cross-National Perspectives* (New York: The Free Press, 1967), p. 50. On the other hand, Lipset reports some data which seem to indicate a decline in class voting among the American electorate from 1936 to 1968: see Lipset, *Revolution and Counter-Revolution: Change and Persistence in Social Structure* (New York: Basic Books, 1968), Table 8-2, pp. 274–275. A change in degree, if not in type of cleavage, seems to be taking place.

[37] See Dennis and McCrone, "Preadult Development of Political Party Identification in Western Democracies," *Comparative Political Studies*, Vol. 3, No. 2 (July, 1970), pp. 243–263. This evidence confirms earlier findings: see Philip E. Converse and Georges Dupeux in Campbell *et al.*, *Elections and the Political Order; cf.* Philip E. Converse, "Of Time and Partisan Stability," *Comparative Political Studies*, Vol. 2, No. 2 (July, 1969), pp. 139–171. In the latter two articles, Converse (and Dupeux) report that individuals who knew their father's party affiliation are more likely to identify with a party themselves than are those whose fathers did not transmit a cue concerning party identification. If citizens with a clear political party identification are relatively unlikely to shift their vote according to underlying values, Table 14 may give a conservative estimate of the impact of value priorities on party choice: the table deals exclusively with those who report a definite party choice themselves *and* received party preference cues from their parents. In addition, however, Converse finds (in "Of Time and Partisan Stability") that older cohorts tend to have relatively strong attachments to given political parties, as a function of the number of years they have been eligible to vote for the political party of their choice in free elections. This suggests the possibility that at least part of the relationship between value preference and party shift may be due to the greater liklihood of older respondents having "acquisitive" values *and* relatively strong party loyalties. This hypothesis might be tested by controlling for age, in addition to the other controls in Table 14. When we do so, the relationship between value preferences and party shift does not seem to disappear, but the highly skewed relationship between age and values reduces the number of cases in some of the cells to the vanishing point. We can apply another sort of test, however, based on cross-national comparisons. Our 1968 data from Britain, France and Germany contain information about the strength of party identification. The pattern varies a good deal from country to country. In the British sample (where the present party system has been established for nearly half a century) intense partisan identification falls off regularly and sharply, as we move from oldest to youngest age group. The oldest British group contains four times as many strong partisan identifiers as does the youngest group. Intense partisanship falls off regularly but less steeply in the German sample (strong identifiers occurring twice as frequently among the oldest group as among the youngest group). So far, this is entirely consistent with the pattern reported by Converse. The French data, however, fit Converse's model only if we regard the present French party system as newly established: partisanship decreases only very slightly in the French sample, as we move from old to young. French teenagers are almost as likely to declare themselves strong partisans as are the 60-year-olds! While at other age levels the French are least likely of the three nationalities to express a strong sense of party identification, among this youngest group they show the *highest* proportion. The relationship between intergenerational party shift and underlying value priorities noted in our French sample cannot readily be attributed to the older cohorts' relatively strong attachment to existing political parties—yet value-linked intergenerational party shift seems to occur to a greater extent in France than in any of the other national samples.

ing pattern of industrial society: the British Labour Party has never been a party of the Left in the same sense as the Marxist parties on the Continent. From the start, it has been a party of moderate reform, rather than one of revolution. Thus, there is less contrast between Labour and Conservative in Britain than between Left and Right on the Continent; an embourgeoisified worker can continue to feel comfortable in voting for the Labour Party[38] while, conversely, a post-bourgeois Englishman has less incentive to switch from Conservative to Labour.

For the time being (as Table 2 indicates), the acquisitive group is much larger than the post-bourgeois group in all of these countries: in case anyone doubted it, the squares outnumber the swingers. In practical terms, this suggests that the potential reservoir of voters who might shift to the Right is larger than the potential base for the New Left. But if our cross-temporal interpretation is correct, this situation is in a process of rapid change. Assuming intracohort stability in value priorities,[39] a projection of changes due to recruitment and mortality based on Table 5 suggests that the two pure groups might reach numerical parity—on the Continent—within the next 20 years. Given the fact that the post-bourgeois types tend to be highly educated, they are likely to be better organized and politically more active than the acquisitive-oriented group. In terms of political effectiveness, the two groups might reach parity within, say, the next 15 years (these projections apply to the European Community countries; Britain appears to lag behind them by about ten years).

The size of the partisan redistribution in France in 1968 may give an idea of the extent to which—under crisis conditions—a similar repolarization might take place in the other countries at the present time. But this process can, of course, be influenced by situational factors, such as political leadership in the given countries. The levels of support for the SPD indicated in our 1970 survey suggest that Willy Brandt, for example, has succeeded in doing what the French Left notably failed to do in 1968—to attract the post-bourgeois group without alienating the acquisitive types.

In Western Europe as a whole, the prospective social base for movements of radical social change appears likely to increase sharply during the next two decades. But in order to be effective, movements seeking radical change must shape their tactics with an awareness of current realities. In view of the wide preponderance which the acquisitives seem to hold over post-bourgeois respondents in Western electorates, a Weatherman-type strategy (for example) not only seems likely to be counterproductive in the short run; to the extent that it had any real impact on the economy, it apparently would tend to be self-defeating in the long run as well.

The new Left-Right continuum resembles the old in that it pits forces of change against those of the status quo—but the values motivating change relate to life styles rather than acquisition, and the social bases supporting change show a corresponding shift. For the time being, the potential social base for the New Left may be a distinct minority. The older value groups are still split, however, and a New Left could be politically effective through alliances with the Old Left which emphasize economic issues—even, to some extent, at the price of playing down some of the expressive issues which are most appealing to the New Left constituency. Conversely, when partisans of the New Left appear to threaten the basic social order (as in France, in May, 1968), they emphasize a cleavage which isolates them from *both* factions of the acquisitive-oriented population: they threaten to upset an apple cart which has for twenty years provided an unprecedented supply of apples. The post-bourgeois group may contend that the apples are sour. They may be right. But the difference in opinion springs from an ingrained difference in tastes.

The present essay has, no doubt, only scratched the surface in the analysis of intergenerational value changes within advanced industrial societies. Further efforts are needed in developing more accurate and more exhaustive measurements of such changes, and in applying these measurements to a longitudinal data base. In this early exploration, we find a fair amount of evidence that our indicators of value priorities tap basic aspects of an individual's belief system: a number of other attitudinal items show relatively great constraint in relation to these value indicators, and the response pattern seems integrated into the social structure in a way which suggests that these values are early-established and relatively stable. Moreover, cross-national differences in value choices have a fit with the economic history of these countries, over the past two generations, which further seems to support this interpretation. It seems at least plausible to conclude that intergenerational change is taking place in the value priorities of West European populations—and that this change may have a significant long-term impact on their political behavior.[40]

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[38] Even relatively affluent English workers are likely to remain staunch supporters of the Labour Party, according to John H. Goldthorpe, David Lockwood, Frank Beckhofer and Jennifer Platt: see *The Affluent Worker: Political Attitudes and Behavior* (Cambridge: Cambridge University Press. 1968). Richard F. Hamilton argues that the same was true of French workers during the Fourth Republic; he may be correct in regard to that period, but our data indicate that the pattern has changed significantly during the Fifth Republic. See Hamilton, *Affluence and the French Worker in the Fourth Republic* (Princeton: Princeton University Press, 1967).

[39] At first glance, the assumption of intracohort stability may seem unrealistic: adult change *does* take place. But, for reasons indicated above, it would probably be rash to assume that the adult cohorts will necessarily become more acquisitive as they age. In view of the uncertainty of the *direction* of possible shifts within adult cohorts, the assumption of intracohort stability may provide at least a useful first approximation.

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[40] These findings seem to contradict some key projections in the literature which focuses on analysis of the future. Herman Kahn and Anthony Wiener, for example, contend that:

> There is a basic, long-term multifold trend toward:
>
> 1. Increasingly sensate (empirical, this-worldly, secular, humanistic, pragmatic, utilitarian, contractual, epicurean or hedonistic) cultures.
> 2. Bourgeois, bureaucratic, "meritocratic," democratic (and nationalistic) elites. . . .

My reading of the data implies that, while these trends may have prevailed until recently, certain aspects may be undergoing a reversal in post-industrial societies. Specifically, I doubt that the elites of these societies will become increasingly bourgeois, meritocratic or nationalistic; or that these cultures are likely to become increasingly pragmatic or utilitarian. Kahn and Wiener make a number of additional projections which *do* strike me as likely to hold true; see *The Year 2000: A Framework for Speculation on the Next Thirty-Three Years* (New York: Macmillan, 1967), p. 7.

The Electoral Impact of Congressional Roll Call Voting

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Although the notion that the American electorate is largely comprised of "rational activists" has been discredited by empirical voting studies, practicing politicians often appear to believe that such an informed public exists. Dramatic evidence of this contradiction is provided by Warren E. Miller and Donald E. Stokes in their study of constituency influence in the House of Representatives.[1] Their interviews with Congressmen indicated that an important linkage in the process of constituency influence is the Representative's belief that his reelection chances vitally hinge on constituency reaction to his record. In their voter interviews, however, only a tiny percentage of the respondents were identified as able to articulate any awareness of their Congressman's policy positions. As Miller and Stokes summarize the obvious paradox:

> Congressmen feel that their individual legislative actions may have considerable impact on the electorate, yet some simple facts about the Representative's salience to his constituents imply that this could hardly be true.[2]

Are Congressmen rewarded and punished at the polls on the basis of their performance even though few constituents follow their performances? Miller and Stokes point out that such constituency reaction is quite possible if the well known "two-step flow" of communications allows rational activists to exert an electoral impact beyond what their numbers would indicate. As information about the Congressman diffuses downward from relatively informed opinion leaders to the mass public, it may be, as Miller and Stokes suggest, that many voters "get simple positive or negative cues about the Congressman which were provoked by his legislative actions but no longer have a recognizable policy content."[3] By responding to such cues, a significant number of voters may act *as if* they are relatively informed about their Congressman's record. As a result, the collective electoral decisions in congressional contests may be more responsive to roll call records than our knowledge about individual voters would indicate.

If the suggestion about the importance of constituency opinion leaders is correct, results of congressional contests may follow the pattern predicted by models of rational voting such as the well known model offered by Anthony Downs.[4] Downs, assuming that a rational informed voter prefers that candidate whose issue position is closest to his own, argues that in a two-party election a candidate will gain votes by moving from the ideological extremes toward the middle of the political spectrum. From Downs's model, as well as from common sense considerations, we would expect moderate Congressmen to be better vote getters than those representing their party's ideological extreme. More specifically, it is hypothesized that:

1. *For a Republican Congressman:* the more conservative he is, the more his vote margin will be reduced.
2. *For a Democratic Congressman:* the more liberal he is, the more his vote margin will be reduced.

This paper tests these hypotheses by means of multivariate analysis of both aggregate data and survey data. At the aggregate level, the district vote for Northern incumbent congressional candidates is analyzed for each of eight recent congressional elections. The supplementary survey analysis, using the Survey Research Center's 1964 Election data, was undertaken to discover which types of voters are most influenced by their Congressman's roll call position. As discussed below, the results of this study strongly indicate that a Republican Congressman's position on the liberal-conservative continuum has a sizeable effect on his vote margin. A less clear pattern emerges for Democrats, perhaps because of the difficulty of detecting sharp ideological differences among Northern Democratic Congressmen.

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[1] Warren E. Miller and Donald E. Stokes, "Constituency Influence in Congress," *American Political Science Review*, 57 (1963), 45–56; Donald E. Stokes and Warren E. Miller, "Party Government and the Saliency of Congress," *Public Opinion Quarterly*, 26 (Winter, 1962), 531–546.

[2] Miller and Stokes, *op. cit.*, p. 54.

[3] Miller and Stokes, *op. cit.*, p. 55.

[4] Anthony Downs, *An Economic Theory of Democracy* (New York: Harper and Row, 1957), Chapter 8.

Measuring the Roll Call Effect

An unavoidable complication in estimating the electoral impact of congressional roll call behavior is that the relationship between roll call performance and constituency voting may be a two-way causal process. If the Representative can create a safer district for himself by employing a moderate voting record, Congressmen from the most competitive districts will be the ones most pressured to employ this strategy. Thus unsafe districts may produce moderate Congressmen, while, as a compensating process, moderate Congressmen may produce safer districts. Clearly, if both hypothesized effects are operating, they could cancel each other out so that the correlation between the two variables would be zero.

In order to disentangle the two effects, two separate measures of district voting are employed—actual congressional election margins (after any roll call effect) and district presidential voting, which serves as an indicator of relative differences in district partisanship. District presidential voting is selected as the indicator of district partisanship since this variable, while highly correlated with congressional voting, is unaffected by the roll call behavior of the district's Congressman. The hypothesized relationship between presidential voting, congressional voting, and roll call performances are presented in the causal model of Figure 1.

By using the presidential vote as the index of district voting, one can infer the effect of constituency voting on roll call behavior free of the possible bias of the hypothesized reverse effect. More important, when the presidential vote is held constant, the relationship between roll call performance and the congressional vote that is partialled out provides an estimate of the electoral impact of the Congressman's roll call performance. To be sure, this estimate may not be totally unbiased, since district presidential voting is not a perfect control for all the confounding electoral forces that jointly affect both the Congressman's roll call behavior and his electoral margin. But since such confounding influence presumably works to obscure rather than exaggerate the roll call effect on voting, the "measurement error" in the control variable would reduce—not magnify—the estimate of the degree to which moderate roll call records produce votes for Congressmen.[5]

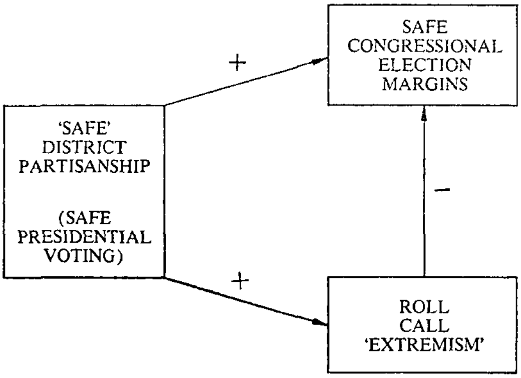
SAFE CONGRESSIONAL ELECTION MARGINS
+
'SAFE' DISTRICT PARTISANSHIP
(SAFE PRESIDENTIAL VOTING)
−
+
ROLL CALL 'EXTREMISM'

Figure 1

Table 1 shows that the simple (product-moment) correlations between roll call positions and district voting for President and Congress follow the pattern consistent with the hypothesized feedback effect of roll call performance on district voting. Among each party's Northern Congressmen, liberalism (defined as *New Republic* ratings[6]) in each of five recent Congresses is shown to be more highly correlated with the district's Democratic vote for President than for Congress. For Democrats, liberalism is correlated at a median value of +.37 with Democratic presidential voting but only

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[5] It is impossible to estimate the exact degree to which uncontrolled confounding influence works against the hypothesized roll call effect in congressional elections. Relevant but unknown factors are the degree to which Congressmen act as electoral opportunists rather than ideologues, the degree to which "opportunists" react to their expected electoral margins as opposed to district interests, and the degree to which Congressmen are sensitive to those causes of their vote margins that are not held constant by the control for district presidential voting. An additional consideration is that the vote for Congress may have a feedback effect on presidential voting. That is, congressional candidates' "coattails" may have some slight effect on the district's presidential vote division. Any such tendency, however, would simply be one additional reason why the roll call effect may be underestimated rather than overestimated. This is because if both the congressional coattail effect and roll call effect are at work, a Congressman's "extreme" roll call position would slightly lower his party's presidential vote as well as his own vote.

[6] Before each Congressional election, *The New Republic* presents a "box-score" of each Representative's roll call performance in the previous Congress on what this liberal journal considers to be important and representative issues. A Congressman's *New Republic* rating is scored here as the frequency of his support for the liberal *New Republic* position on these selected items. (Failure of the Congressman to take a position on an issue is scored as half a vote for each alternative.) The number of roll calls included on this scale for a particular Congress ranges from nine to sixteen and includes a cross section of social welfare, civil rights, and foreign policy issues. For 1960, because *The New Republic* failed that year to present roll call records for the outgoing Congress, liberalism scores given by the Americans for Democratic Action are substituted.

Table 1. Simple Correlations Between Congressional Liberalism and the District Democratic Vote for President and for Congress[a]

Congress	Election Year	Republicans			Democrats		
		N	Pres.	Cong.	N	Pres.	Cong.
1951–52	1952	(154)	+.42	+.04	(74)	+.37	+.17
1955–56	1956	(169)	+.16	−.07	(95)	+.35	+.30
1959–60	1960	(124)	+.54	+.10	(133)	+.43	+.27
1963–64	1964	(142)	+.48	−.22	(117)	+.22	+.10
1967–68	1968	(136)	+.58[b]	−.10	(126)	+.49[b]	+.24
	Median		+.48	−.07		+.37	+.24

[a] All northern incumbent congressional candidates with electoral opposition in the given election year are included in this analysis. The construction of the liberalism index employed is discussed in footnote 6.

[b] For 1968, the Democratic presidential vote is calculated as the Democratic percentage of the three-party vote.

+.24 with Democratic congressional voting. The comparable median correlations for Republicans are an even more divergent +.48 and −.07. That is, although the most conservative Republicans in the House tend to represent the constituencies that are most Republican in their presidential voting, they are not necessarily the ones with the safest electoral margins.[7]

In general, it would appear from Table 1 that a district that is "safe" for the Congressman's party in its presidential voting makes a Northern Congressman more likely to vote with his party's ideological extreme. But the lack of a consistent correlation between districts that are safe in their congressional voting and roll call "extremism" may be an initial indication that an extreme roll call position makes the Congressman's electoral margin less safe. If extremely conservative Republicans and extremely liberal Democrats receive the hypothesized punishment at the polls, they should get smaller electoral margins than would be expected on the basis of their districts' presidential voting. The next step, therefore, is to see whether such a negative relationship emerges between roll call extremism and safe electoral margins when district presidential voting is held constant.

Although the simple matching of cases is also employed, the method this paper most heavily relies on in estimating the electoral effect of congressional roll call positions is multiple regression analysis. For each election from 1952 through 1968, with the exception of 1966 (see note 9, below), multiple regression equations were derived for both Republican and Democratic incumbent congressional candidates from Northern districts.[8] The dependent variable of the equations was the incumbent's percentage of the two-party vote. The explanatory variable was the degree to which the incumbent's issue stance in the previous Congress represents his party's extreme—conservative for Republicans and liberal for Democrats. Various alternate indices of congressional roll call voting were employed. The one measure that was used for Congressmen of both parties in each of the eight elections is the Congressman's *New Republic* rating for the previous Congress.

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[7] This pattern of correlations is consistent with the relationships between district voting and roll call performance that have been previously reported. Negligible correlations between roll call "extremism" and measures of district "safeness" that are based on congressional election results have been reported in Wayne Shannon, "Electoral Margins and Voting Behavior in the House of Representatives: The Case of the Eighty-Sixth and Eighty-Eighth Congresses," *Journal of Politics,* 30 (November, 1968), 1028–1045; Loren K. Waldman, "Liberalism of Congressmen and the Presidential Vote in their Districts," *Midwest Journal of Political Science,* 11 (February, 1967), 73–85; and Lewis A. Froman, Jr., *Congressmen and their Constituencies* (Chicago: Rand McNally, 1963), pp. 92–95. (Although Froman claims to have found an appreciable relationship, Shannon has shown that Froman's data do not support this conclusion.) A positive relationship between Democratic presidential voting and roll call liberalism has been reported in Milton C. Cummings, Jr., *Congressmen and the Electorate,* (New York: The Free Press, 1966), pp. 114–116; in Waldman, *op. cit.;* and (for the South) in Thomas A. Flinn and Harold Wolman, "Constituency and Roll Call Voting: The Case of Southern Democratic Congressmen," *Midwest Journal of Political Science,* 10 (March, 1966), 192–199.

One previous examination of the relationship between congressional election outcomes and roll call liberalism with district presidential voting held constant is a brief analysis by Cummings (*op. cit.,* pp. 118–119) of the 1948 election. Cummings grouped Republican incumbent candidates on the basis of similarity of district presidential voting and found an intriguing tendency for the average liberalism score (defined as ADA ratings) of the winners to be higher than that of their counterparts in the same group who were defeated. Further evidence of a roll call effect can be found in Robert A. Schoenberger, "Campaign Strategy and Party Loyalty: The Electoral Relevance of Candidate Decision-Making in the 1964 Congressional Elections," *American Political Science Review,* 63 (June, 1969), 515–520. Schoenberger's data indicate that Republican Congressmen who supported Goldwater prior to the 1964 convention (presumably the most conservative) represented districts that were more Republican in their presidential voting but less Republican in their congressional voting than the districts of their counterparts who did not support Goldwater at an early date.

[8] The "North" is defined so as to exclude the Border States of Kentucky, Maryland, Missouri, Oklahoma, and West Virginia, in addition to the former Confederate States.

The procedure for controlling for presidential voting in the regression equations was straightforward. For all presidential years except 1968, district presidential voting was controlled by simply entering the two-party vote division for President in the given year into the equations. Because of 1968's three-way presidential race, both the Wallace and Humphrey percentages of the three-party vote were included for the 1968 equations. In the equations for the 1954 and 1958 midterm elections, the presidential vote divisions in each of the surrounding presidential years were entered. For 1962, however, the vote for President in only 1964 was included because the 1960 presidential vote was unavailable for several districts reapportioned prior to 1962.[9]

One additional control variable, entered into a second set of equations, was the Congressman's residual vote from the previous election. The Congressman's residual vote is the deviation of the Congressman's vote in the previous election from that predicted by the initial equation for that year. This variable, which on the average increases the coefficient of determination (R^2) by about two-thirds, may be thought of as a measure of the Congressman's previous vote getting ability apart from that portion attributable to his roll call position.[10]

Although not included as an independent variable in the equations, one final and important variable that was controlled is the ballot form. This variable was held constant by the derivation of separate regression equations for Congressmen from states employing the "single choice" ballot and for those from states employing the "multiple choice" ballot. This control was introduced because the ballot form was found to have an interaction effect on the relationship between presidential voting and congressional voting. The "single choice" ballot, employed in about half the states, facilitates straight ticket voting by allowing a straight party vote to be made by a single pull of a party lever or mark of a party ballot. The "multiple choice" ballot, in use in the remaining states, encourages split ticket voting by forcing the voter to vote separately for each office.[11] By encouraging split ticket voting, the multiple choice ballot helps insulate House election results from changes in the political climate. This insulation is reflected in a sharp tendency for the correlation between the district congressional vote and district presidential vote to be lower for multiple choice districts than for single choice districts.[12] The ballot form's interaction effect, which can be traced to the ballot impact at the individual level, is that the regression of the congressional vote on the presidential vote is steeper for single choice districts than for multiple choice districts.[13] Because of this complication, the derivation of separate equations for single choice districts and for multiple choice districts was necessary as a means of maximizing the effectiveness of the presidential vote as a control for the "expected" congressional vote.[14]

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[9] For the 1962 equations, a few districts in Connecticut, Michigan, and Wisconsin were excluded from analysis because the 1964 presidential vote was unavailable. This was due to redistricting between 1962 and 1964 and the drawing of district boundaries in a way that did not conform to county lines. Redistricting in Washington between 1956 and 1958 caused the exclusion of four constituencies in that state from the 1958 analysis. The extensive reshuffling of district lines between 1964 and 1968 was the reason why no analysis was made of the 1966 election.

[10] Of course no equations with the residual vote as an independent variable were calculated for 1952 and 1968 because there was no measure of the residuals from the immediately prior 1950 and 1966 election years. When the residual vote was controlled, the number of cases was slightly reduced because of the exclusion of districts with freshman Congressmen and Congressmen who faced no major party opposition in the previous election. In the equations from which the residual vote was derived, the measure of roll call performance that is employed is the Congressman's *New Republic* rating. Two independent variables were included in the equations from which the residual vote values were derived, but were dropped from the equations from which the roll call effect estimates in this paper were taken. These two variables are the Congressman's seniority and an index of state forces (the coattail effects of statewide contests for Governor or U.S. Senator). Seniority, as measured by the number of years served in Congress, was found to have neither a strong nor a consistent "effect" on congressional election margins. Estimated from the average of the regression coefficients for seniority, one decade of seniority offers the Congressman no more than an additional one per cent increase in his share of the two-party vote. The index of state forces is the set of residuals from equations with the vote for statewide office regressed on the state's presidential vote. When the index of state forces is included as an independent variable in the equations predicting the district congressional vote, the regression coefficients for this variable, although tending in the expected direction, are quite erratic. The estimates of the roll call effect when either or both of these rejected variables are controlled are virtually identical to those without these controls that are reported in this paper.

[11] See Angus Campbell and Warren Miller, "The Motivational Basis of Straight and Split Ticket Voting," *American Political Science Review*, 51 (June, 1957), 293–312.

[12] For districts with Republican incumbent candidates, the median correlations between the presidential and congressional vote divisions over the five presidential elections is + .51 for single choice districts but only + .28 for multiple choice districts. A less dramatic difference in median correlations of + .88 vs. + .83 is found for districts with Democratic in-

The Electoral Impact of Republican Roll Call Performances

The findings for Republican Congressmen are presented first. Table 2 displays the results of the regression analysis using *New Republic* ratings as the index of roll call conservatism. When possible (that is, except for 1952 and 1968), the findings in this table were based on equations that include the control for the incumbent candidate's residual vote in the previous election. (This reduces the sample sizes somewhat, because Congressmen who ran unopposed or were not incumbents at the time of the previous election were excluded from analysis when the residual vote is controlled.) The multiple correlation coefficients (*R*'s), which as expected were highest for single choice districts, indicate the degree of correlation between the actual congressional vote and the predicted values from the regression equations. The partial correlations indicate the degree of association between the Congressman's conservatism and his vote margin when the control variables are held constant. The regression coefficients (*b*'s), or slopes, indicate the estimated change in the Republican two-party vote for Congress accompanying a unit's change in the Representative's conservatism. To make the slopes comparable from year to year, they were calculated using z-scores, or standard deviation units, as the units of conservatism. (The standard deviation is calculated from the distribution of conservatism scores for the entire sample of Republican incumbent candidates for the given election year.) Thus the slopes indicate the estimated difference in the Republican percentage of the two-party vote associated with a difference of one standard deviation (for Republicans) along the ideological continuum.[15]

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cumbent candidates. (The presidential vote division for 1968 is the Humphrey percentage of the three-party vote.) A similar ballot effect is found on the correlations between congressional voting in midterm elections and the presidential vote in the surrounding presidential years. These correlational differences suggest that candidates' characteristics—including roll call performances—may have a greater impact on congressional election results when the multiple choice ballot is used. But these correlational differences also indicate that the multiple choice ballot lowers the adequacy of presidential voting as an indicator of the Congressman's expected vote margin. Consequently the least confounding influence and the most accurate estimates of the roll call effect should be found for single choice districts.

[13] For presidential years, the average slopes (b's) of the regression of the congressional vote on the presidential vote are (with *New Republic* ratings held constant) as follows: for districts with Republican incumbent candidates, +.76 with a single choice ballot and +.46 with a multiple choice ballot; for districts with Democratic incumbent candidates, +.80 with a single choice ballot and +.72 with a multiple choice ballot. The tendency is similar for midterm years. There appears to be no consistent additive effect of the ballot form, since the points of intersection of the single choice and multiple choice slopes do not deviate systematically from the intersections of the two mean vote divisions for districts with incumbent candidates of the same party.

An explanation for the ballot form's impact on the regression of the congressional vote on the presidential vote can be drawn from the ballot form's impact on the propensity toward split ticket voting. Consider the following hypothetical example in which the ballot form is assumed to have the maximum possible impact. Suppose that in single choice districts the individual voter's congressional vote is perfectly dependent on his presidential choice (i.e., a perfectly operating presidential coattail effect). If so, at the district level there would be a one to one correspondence between the vote division for President and the vote division for Congress. The regression coefficient for this hypothetical relationship would be a steep 1.0. Now suppose that the multiple choice ballot encourages perfect split ticket voting so that there is absolutely no relationship at the individual level between the voter's presidential and congressional choices. If so, there would be no relationship at the district level (the expected congressional vote division in each district would be the mean) and the slope would be zero. The same logic applies to midterm elections also, with the presidential vote divisions in surrounding presidential years indicating the distribution of district partisanship and congressional voting at the individual level assumed to be more consistent with party identification when the single ballot is employed.

[14] An alternate means of controlling for the ballot form that would not involve separate equations for single choice and multiple choice districts would be the inclusion of the ballot form and an interaction term (the product of the ballot form and the presidential vote) as variables in the equations. On this method of controlling for interaction in regression analysis, see Norman Draper and Harry Smith, *Applied Regression Analysis* (New York: John Wiley and Sons, 1966), pp. 140–141. The hypothesized ballot impact on both the roll call effect and the degree to which confounding influence is controlled (see footnote 12, above) was the major reason this method of control was not employed.

[15] As a rule, regression coefficients should be reported in unstandardized form unless the metric of one or both of the variables lacks substantive meaning. Since the units of roll call indices applied to different Congresses are not directly comparable (because

Table 2. Relationship Between the Roll Call Conservatism[a] and Electoral Margins of Republican Congressmen

Controlling for District Presidential Voting and Congressmen's Residual Vote in Previous Election

Election Year	Districts with Single Choice Ballot				Districts with Multiple Choice Ballot			
	N	Regression Coefficient	Partial Correlation	Multiple Correlation	N	Regression Coefficient	Partial Correlation	Multiple Correlation
1952[b]	(74)	−1.33 (.31)[c]	−.45	.915	(80)	−2.79 (.63)[c]	−.47	.689
1954	(71)	−1.28 (.40)	−.37	.812	(63)	−0.39 (.58)	−.09	.744
1956	(63)	−1.57 (.34)	−.51	.898	(90)	−0.85 (.41)	−.22	.780
1958	(59)	−1.69 (.67)	−.33	.692	(68)	−0.95 (.47)	−.24	.710
1960	(44)	−1.15 (.35)	−.47	.877	(55)	−1.10 (.71)	−.21	.703
1962	(40)	−1.71 (.62)	−.42	.585	(63)	−0.68 (.79)	−.11	.463
1964	(48)	−2.77 (.72)	−.50	.683	(65)	−3.06 (.74)	−.47	.637
1968[b]	(61)	−2.64 (.64)	−.48	.708	(75)	−1.54 (.89)	−.20	.283
Mean		−1.77				−1.42		
Median			−.46	.760			−.22	.696

[a] *New Republic* ratings.

[b] For 1952 and 1968 there is no control for the Congressman's residual vote in the previous election.

[c] Figures in parentheses indicate the standard errors of the regression coefficients.

All sixteen partial correlations presented in Table 2 are in the expected negative direction, indicating that with relevant variables held constant a Republican Congressman gets fewer votes when he has a relatively conservative roll call position. For single choice districts the median partial correlation is −.46. For multiple choice districts, for which a smaller portion of the variance is controlled, the median partial correlation is −.22. The average slopes are −1.77 for single choice districts and −1.42 for multiple choice districts. The average of these averages is −1.60, which suggests that a Republican Congressman gets about 1.6 per cent less of the two-party vote than he would have if he were one standard deviation less conservative. Since about 95 per cent of the cases in a distribution fall within an interval of four standard deviations, a reasonable estimate is that an unusually liberal Republican Representative normally gets at least 6 per cent more of the two-party vote (4 × 1.6) than his extreme conservative counterpart would in the same district.

Quite similar results were obtained using two additional indices of roll call conservatism—*Congressional Quarterly*'s Party Unity scores and (for the Kennedy-Johnson Congresses) Administration Opposition scores.[16] These two indices are highly correlated with each other and with the *New Republic* ratings.[17] The results for 1962, 1964, and 1968 using each of the three indices (calculated without the residual vote control) are compared in Table 3.[18]

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the selected items are different), unstandardized coefficients would seem inappropriate. However, the standarization of the units of the roll call indices, but not those of the easily interpretable dependent variable, overcomes this difficulty. Assuming that the spread of a party's ideological distribution in Congress is constant over time, standardization of the roll call index makes the slopes derived for different election years directly comparable. This procedure also makes the slopes using alternate roll call indices for the same election year comparable. On choosing whether or not to standardize the slopes, see Hubert M. Blalock, "Causal Inferences, Closed Populations, and Measures of Association," *American Political Science Review*, 61 (March, 1967), 130–136.

[16] Scores on these two indices were computed as the frequency of party voting or opposition to the Democratic Administration on only those relevant roll calls on which the Congressman took a position. This method leaves the scores unaffected by rates of absenteeism.

[17] Over eight Congresses, the median correlation between Party Unity scores and *New Republic* ratings is +.78. For the last three Congresses analyzed, the median correlations between the indices are: *New Republic*-Party Unity, +.87; *New Republic*-Administration Opposition, +.78; and Party Unity-Administration Opposition, +.92. Unlike the other two indices, which have distributions that are somewhat negatively skewed, Administration Opposition scores approximate a normal distribution, and for that reason may offer the preferred index for those years it can be employed.

[18] Over all eight elections, the average of the sixteen slopes using the Party Unity index is −1.5, which is the same as the average slope using the *New Republic* index without the residual vote control. Since alternate indices that are imperfectly correlated with each other produce equivalent slopes, each index would appear to have about the same amount of measurement error. Since random measurement error in the independent variable may depress the magnitudes of regression coefficients, imperfection in the measure of "conserva-

Table 3. Relationships Between Three Indices of Roll Call Conservatism and the Electoral Margins of Republican Congressmen in 1962, 1964, and 1968

Controlling for District Presidential Voting

	Districts with Single Choice Ballot				Districts with Multiple Choice Ballot			
Election Year	N	Roll Call Index	Regression Coefficient	Partial Correlation	N	Roll Call Index	Regression Coefficient	Partial Correlation
1962	(55)	New Repub.	−1.80 (.57)[a]	−.41	(79)	New Repub.	−1.03 (.80)[a]	−.15
		P'ty Unity	−1.94 (.63)	−.40		P'ty Unity	−0.75 (.76)	−.11
		Adm. Opp.	−1.58 (.68)	−.33		Adm. Opp.	−1.46 (.81)	−.20
1964	(61)	New Repub.	−2.09 (.91)	−.36	(81)	New Repub.	−2.71 (.71)	−.40
		P'ty Unity	−2.73 (.65)	−.48		P'ty Unity	−2.79 (.72)	−.40
		Adm. Opp.	−2.80 (.63)	−.50		Adm. Opp.	−3.16 (.74)	−.44
1968	(61)	New Repub.	−2.64 (.64)	−.48	(75)	New Repub.	−1.54 (.89)	−.20
		P'ty Unity	−2.95 (.76)	−.46		P'ty Unity	−2.40 (.85)	−.32
		Adm. Opp.	−1.50 (.70)	−.27		Adm. Opp.	−2.35 (.85)	−.31

[a] Figures in parentheses indicate the standard errors of the regression coefficients.

Another means of demonstrating the roll call effect is simply matching relatively liberal and relatively conservative Republican Congressmen from politically similar districts and then comparing their vote margins. Although the number of cases is sharply curtailed, this procedure has the advantages of allowing an effective check for the possible confounding influence of geographic region and affords a somewhat more intimate grasp of the data than regression analysis allows. In order to obtain two groups of Republican Congressmen who were ideologically divergent but had constituencies as alike as possible in their normal voting, Republican incumbent candidates were paired if they were at least one standard deviation apart in ideology and represented neighboring districts in the same state that were similar in their presidential voting. The most conservative member of each matched pair was placed in the "conservative" group and the most liberal in the "liberal" group. As a simple measure of the average difference in the vote getting ability of the two groups, the mean difference in their vote lead over the Republican vote for President was used.[19] To obtain an estimate of the vote difference that one standard deviation of conservatism makes (the analogue to the regression coefficients), the difference in the index of vote getting ability was divided by the mean roll call difference (in standard deviation units) of the two groups. For 1962 and subsequent election years, conservatism was measured by Administration Opposition scores; for the earlier years *New Republic* ratings were used.[20]

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tism" is a further reason, in addition to the problem of confounding influence, why the roll call effect may be underestimated.

[19] For 1968, the vote for President is measured as Nixon's percentage of the three-party vote. For the midterm elections, the vote for President is measured as the average of the two-party vote divisions in the two surrounding presidential years. When the presidential vote in only one of the surrounding presidential years is available for one or both of the matched cases, only the vote for the one year is used.

[20] To be matched, two districts with Republican Congressmen at least one standard deviation apart on the appropriate roll call index had to meet the following minimum criteria:

1. They are within the same state and either adjacent or both entirely within the same SMSA
2. They are within 5 per cent of each other in the two-party division of the presidential vote (with the presidential vote measured by the criteria indicated in footnote 19)
3. The total two-party vote in each district totals at least 97% of the total votes cast.

When more than one possible combination of matched pairs of districts were possible in the same state, the following criteria were applied, in descending order, until the tie was broken.

1. Maximize the number of pairs
2. Maximize the minimum difference in roll call performance
3. Minimize the maximum difference in presidential voting.

From the set of matched pairs obtained by this method, we excluded those in which the Republican presidential vote in the district with the more conservative Congressmen exceeded that in its matched district by more than 3.5 per cent. This made the mean Republican percentages of the two-party presidential vote in the two sets of districts equal (summed over all cases) to within 0.1 per cent of each other. Before this correction, the presidential vote in the districts with "conservatives" was over one per cent more Republican than that in the districts with "liberals." Since

For illustration, a detailed presentation of the results for the 1964 election using the matching technique is shown in Table 4. In fourteen of the sixteen pairs of Republican incumbent candidates that were matched for the 1964 election, the more conservative Congressman's lead over the Goldwater vote was smaller than that of his more liberal counterpart. Although the mean Goldwater vote for the two groups of districts were virtually identical (42.0 per cent for the conservatives and 42.1 per cent for the liberals), the mean congressional vote margins for the two groups were quite different. The average vote for the "conservatives" was a perilously marginal 51.1 per cent while the average vote for the "liberals" was a comparatively safe 56.1 per cent. The average lead over Goldwater was 4.9 per cent greater for the "liberal" group than for the "conservative" group. Since the mean ideological distance between the two groups is 1.4 standard deviations, the estimated vote difference per standard deviation of conservatism was −4.9 per cent /1.4 or 3.5 per cent of the two-party vote. This estimate is slightly higher than those for 1964 based on the regression analysis.

The results for all eight election years and the results combined across years are summarized in Table 5. (For the combined results, the average vote leads and standardized issue positions are used when the same pair of Congressmen are matched for more than one election year.) For each election the "conservatives" are the poorer vote getters in more than half the cases. Overall, the "conservative" member of the pair is the poorer vote getter in 70 per cent of the 116 cases, with an average vote difference per standard deviation of conservatism of 1.7 per cent. That is, a Republican Congressman appears to get 1.7 per cent less of the two-party vote than if he were one standard deviation more liberal. This estimate is quite similar to that of 1.6 per cent based on the average of the regression coefficients. Curiously, the effect estimated by matching is greater for multiple choice districts (−2.1 per cent) than for single choice districts (−1.4 per cent). This reversal of the pattern of the regression based estimates is more in line with common sense since the multiple choice ballot's encouragement of split ticket voting ought to stimulate the election impact of candidate characteristics such as roll call positioning.

The Electoral Impact of Democratic Roll Call Performances

For Democratic Congressmen, the evidence that moderate (less liberal) candidates are better vote getters is somewhat elusive. Table 6 displays the estimates of the roll call effect for Democrats from the regression equations with *New Republic* ratings as the liberalism index and with the control for the Congressmen's residual vote in the previous election. The format of Table 6 is the same as that of Table 2, above, for Republicans, except that here the units of the independent variable for the calculation of the slopes are the standard deviations for Democrats rather than for Republicans. As with the similar set of equations for Republican Congressmen, the multiple correlation coefficients tend to be higher for single choice districts than for multiple choice districts.

Twelve of the 16 partial correlations and slopes presented in Table 6 are in the expected negative direction. The slopes are quite erratic, especially for single choice districts, for which the average slope over eight elections is virtually zero. Only for multiple choice districts is the slope consistently negative (with one exception), averaging −1.12 over eight elections. Using the average of the two averages (−0.56), we estimate that a Democratic Congressman gets a mere 0.56 per cent less of the two-party vote than he would have if he were one standard deviation less liberal.

The erratic nature of the slopes may in part be a function of the inadequacy of the *New Republic* ratings as an index of liberalism for Northern Democrats. The problem with the index is that the spread of scores is small, with most Northern Democrats clustered in the range of near-perfect to perfect liberal voting records on the issues selected by the *New Republic*. Thus, although the *New Republic* index is capable of identifying the most conservative Northern Democrats, it discriminates hardly at all between the moderately liberal and the very liberal.

Unfortunately, when only Northern Democrats are compared, the various *Congressional Quarterly* indices not only share the inadequacies of the *New Republic* index but also lack its virtue of content validity as an index of liberalism.[21] Consequently, in order to employ an additional index of roll call liberalism that ade-

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the Congressman's lead over his presidential ticket is inversely related to his party's share of the presidential vote, the correction was necessary to insure against a slight spurious inflation of the estimated roll call effect.

[21] For the 1963–1964 Congress, Party Unity and Administration Support Scores of Northern Democrats were found to be correlated at a weak +.38 and +.47, respectively, with *New Republic* ratings. Their correlations with the Liberal Issue scores (discussed below) are an even less substantial +.18 and +.17.

Table 4. The Difference in the Vote for Matched "Conservative" and "Liberal" Republican Congressmen in 1964

	Congressman	State and District	Conservatism (z-Score)	Rep. % Two-Party Vote			
				Pres.	Cong.	Cong. Minus Pres.	Diff.
C	Clawson	Calif. 23	+0.8	41.4	55.4	+14.0	− 8.7
L	Hosmer	Calif. 32	−0.3	46.2	68.9	+22.7	
C	Findley	Ill. 20	+1.1	42.9	54.8	+11.9	+ 0.3
L	Springer	Ill. 22	−0.6	41.4	53.0	+11.6	
C	Kyl	Iowa 4	+0.6	35.2	46.4	+11.2	− 0.9
L	Schwengel	Iowa 1	−0.6	36.9	49.0	+12.1	
C	Gross	Iowa 3	+1.5	39.7	50.1	+10.4	− 0.7
L	Bromwell	Iowa 2	+0.3	36.7	47.8	+11.1	
C	Dole	Kans. 1	+1.1	44.4	51.2	+ 6.8	− 6.8
L	Shriver	Kans. 4	−0.1	45.8	59.4	+13.6	
C	Skubitz	Kans. 5	+0.9	45.7	56.4	+10.7	− 6.1
L	Ellsworth	Kans. 3	−0.7	45.4	62.2	+16.8	
C	Johansen	Mich. 3	+1.7	39.3	47.3	+ 8.0	−10.5
L	Ford	Mich. 5	−0.0	42.7	61.2	+18.5	
C	Hutchinson	Mich. 4	+1.1	44.1	54.3	+10.2	− 3.3
L	Griffin	Mich. 9	−0.3	43.9	57.4	+13.5	
C	Beerman	Neb. 1	+1.9	45.8	48.7	+ 2.9	− 5.8
L	Cunningham	Neb. 2	+0.8	44.4	53.1	+ 8.7	
C	Grover	N.Y. 2	−0.9	43.5	51.6	+ 8.1	− 8.2
L	Halpern	N.Y. 6	−2.5	40.8	57.1	+16.3	
C	Derounian	N.Y. 3	+0.3	39.1	49.3	+10.2	− 8.0
L	Reid	N.Y. 26	−0.9	36.7	54.9	+18.2	
C	St.George	N.Y. 27	+0.9	37.3	48.4	+11.1	+ 6.7
L	Barry	N.Y. 25	−0.9	39.4	43.8	+ 4.4	
C	Short	N.Dak. 2	+1.3	43.3	47.5	+ 4.2	− 7.4
L	Andrews	N.Dak. 1	−0.2	40.8	52.4	+11.6	
C	Ashbrook	Ohio 17	+2.0	42.5	51.5	+ 9.0	− 9.0
L	Betts	Ohio 8	+0.6	43.8	61.8	+18.0	
C	Goodling	Pa. 19	+0.8	40.6	49.2	+ 8.6	− 4.8
L	Whalley	Pa. 12	−0.6	45.2	58.6	+13.4	
C	Berry	S.Dak. 2	+1.1	47.0	56.0	+ 9.0	− 4.9
L	Reifel	S.Dak. 1	−0.0	43.6	57.5	+13.9	
C			+1.0	42.0	51.1	+ 9.1	− 4.9
L	Mean		−0.4	42.1	56.1	+14.0	
	Difference		+1.4	−0.1	−5.0	− 4.9	

−3.5% = Vote Diff. per Standard Deviation of Conservatism.
$1.4\overline{)-4.9\%}$

Table 5. Roll Call Effect on the Vote for Republican Congressmen, Estimated by Matching of Cases

Year	N	% of Cases Liberal is Better Vote Getter	Mean Vote Difference per SD Conservatism
1952	(5)	100%	−2.2%
1954	(28)	68	−1.3
1956	(30)	60	−1.2
1958	(13)	54	−0.7
1960	(13)	69	−2.5
1962	(26)	69	−2.1
1964	(16)	88	−3.5
1968	(15)	80	−1.9
All Years	(116)[a]	70[b]	−1.7
SC Ballot	(52)	65	−1.4
MC Ballot	(64)	73	−2.1

[a] The sample size for "all years" is smaller than the sum of yearly sample sizes because the estimated effects for pairs of Congressmen who were included for more than one election year are averaged in the combined results for all years.

[b] The equivalent of 70 per cent "heads" in 116 flips of a coin is statistically significant beyond the .01 level. All 116 data points are not entirely independent of each other, however, because some Congressmen are included more than once when matched with different counterparts for different elections. No confidence interval can be calculated for the estimate of the magnitude of the roll call effect because this estimate is calculated as the *mean* vote difference per *mean* standardized roll call difference.

quately discriminates among Northern Democrats, a new index was constructed. This new index is the frequency of liberal responses on roll call votes which were judged to tap the liberal-conservative dimension and on which at least ten per cent of the Northern Democrats took the conservative position. Since the roll calls selected were those on which it was "difficult" for a Northern Democrat to take a liberal position, this scale is referred to as the "Liberal Issue" index. Liberal issue scores were calculated for Northern Democrats for the Congresses preceding the 1962, 1964, and 1968 elections. Reflecting the types of issues that have most divided Northern Democrats recently, the roll calls comprising this index deal with civil rights and foreign policy more often than with economic issues.[22]

Table 7 shows that the slopes using the Liberal Issue index are no more consistent than those using the *New Republic* index. In fact, half of the six slopes derived with the new index are positive, and half are negative. Thus, even using the improved measure of Democratic liberalism, regression analysis provides no statistical evidence that a Democratic Congressman loses votes by liberal roll call voting.

Some evidence of the expected roll call effect for Democrats is provided by the matching technique. Relatively liberal and relatively con-

Table 6. Relationship Between the Roll Call Liberalism[a] and Electoral Margins of Democratic Congressmen

Controlling for District Presidential Voting and Congressmen's Residual Vote in Previous Election

Election Year	Districts with Single Choice Ballot				Districts with Multiple Choice Ballot			
	N	Regression Coefficient	Partial Correlation	Multiple Correlation	N	Regression Coefficient	Partial Correlation	Multiple Correlation
1952[b]	(32)	−1.26 (.43)[c]	−.48	.954	(42)	−2.88 (1.02)[c]	−.41	.904
1954	(24)	+0.55 (.80)	+.16	.833	(27)	−1.06 (.74)	−.29	.972
1956	(28)	+0.01 (.51)	+.00	.933	(37)	−1.25 (1.15)	−.19	.882
1958	(32)	+1.45 (.71)	+.37	.939	(39)	−1.29 (.50)	−.41	.915
1960	(36)	−0.28 (1.42)	−.04	.969	(41)	+0.26 (1.08)	+.04	.885
1962	(38)	−0.02 (.75)	−.00	.821	(48)	−0.10 (1.00)	−.02	.732
1964	(34)	−0.11 (.91)	−.02	.925	(48)	−0.26 (.54)	−.06	.890
1968[b]	(50)	−0.37 (.86)	−.06	.836	(76)	−2.35 (.78)	−.33	.776
Mean		−0.00				−1.12		
Median			−.01	.929			−.26	.887

[a] *New Republic* ratings.

[b] For 1952 and 1968 there is no control for the Congressman's residual vote in the previous election.

[c] Figures in parentheses indicate the standard errors of the regression coefficients.

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[22] The median correlation between Liberal Issue scores and *New Republic* ratings is +.64. The number of roll calls included in the Liberal index are: *1961–1962*, 12; *1963–1964*, 16; *1967–1968*, 62. The large increase in the number of includable roll calls for the 1967–1968 Congress appears to reflect the increased division within the Northern wing of the Democratic party. A list of the roll calls included in the Liberal Issue index is available from the author upon request.

Table 7. Relationships Between Two Indices of Roll Call Liberalism and the Electoral Margins of Democratic Congressmen in 1962, 1964, and 1968

Controlling for District Presidential Voting

Election Year	Districts with Single Choice Ballot				Districts with Multiple Choice Ballot			
	N	Roll Call Index	Regression Coefficient	Partial Corre-lation	N	Roll Call Index	Regression Coefficient	Partial Corre-lation
1962	(42)	New Repub. Lib. Issue	−0.55 (.94)[a] +0.85 (1.00)	−.12 +.14	(63)	New Repub. Lib. Issue	+0.13 (.87)[a] +0.34 (.76)	+.02 +.06
1964	(46)	New Repub. Lib. Issue	+0.09 (.91) +0.69 (.75)	+.02 +.16	(71)	New Repub. Lib. Issue	−0.61 (.51) −0.37 (.58)	−.15 −.08
1968	(50)	New Repub. Lib. Issue	−0.37 (.86) −0.00 (1.01)	−.06 −.01	(76)	New Repub. Lib. Issue	−2.35 (.78) −1.45 (.86)	−.33 −.19

[a] Figures in parentheses indicate the standard errors of the regression coefficients.

servative Democrats were matched together by the same technique employed for Republicans.[23] For 1962 and subsequent election years the matched Congressmen from similar neighboring districts had to be at least one standard deviation apart on the Liberal Issue index. For the earlier years the paired Democrats were at least one standard deviation apart on the *New Republic* index. The results are summarized in Table 8. When the results were combined across election years, thirty-six pairs of matched Congressmen were obtained. Although the tendency is not statistically significant, in 61 per cent of the thirty-six matched pairs the more conservative Democrat was the better vote getter as measured by the difference in the two Congressmen's vote lead over their presidential ticket. The estimated vote loss per standard deviation of liberalism, which does not vary with the ballot form, is −1.2 per cent, which is considerably greater than the average of the regression based estimates.

Table 8. Roll Call Effect on the Vote for Democratic Congressmen Estimated by Matching of Cases

Year	N	% of Cases Conserva-tive is Better Vote Getter	Mean Vote Difference per SD Liberalism
1952–1958	(13)	54	−0.5%
1960	(8)	50	−2.0
1962	(10)	70	−1.7
1964	(9)	55	−0.4
1968	(5)	80	−2.6
All Years	(36)[a]	61[b]	−1.2
SC Ballot	(17)	65	−1.1
MC Ballot	(19)	58	−1.3

[a] The sample size for "all years" is smaller than the sum of yearly sample sizes because the estimated effects for pairs of Congressmen who were included for more than one election year are averaged in the combined results for all years.

[b] Not statistically significant at the .05 level.

It is difficult to sort out from the conflicting estimates of the regression and matching analyses the degree to which liberal Democrats face electoral reprisals; nevertheless it is safe to say that there is much less evidence that voters punish liberal Democrats than there is that voter punish conservative Republicans. It may be that voters do not differentiate much between relatively liberal and relatively conservative Northern Democrats simply because the Northern wing of the Democratic party has been more ideologically homogeneous than the Republicans. Until the most recent Congresses, Northern Democrats have been fairly united on the most salient issues that have come up for roll call voting while Republican Representatives have been more visibly divided.[24] Consequently, even the most attentive voter must have had greater difficulty detecting shades of opinion among Northern Democrats than among Northern Republicans.

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[23] For the matched Democratic Congressman for 1968, the presidential vote is measured as the Humphrey percentage of the three-party vote. Otherwise the procedure is the same as for the matching of Republicans. Exclusion of the originally matched pairs in which the liberal's district is more than 3.5% more Democratic produces a virtual equal balance in the mean presidential voting of the two groups, as this correction did in the matching of Republicans.

[24] For a detailed demonstration of this partisan difference, see David Mayhew, *Party Loyalty Among Congressmen* (Cambridge: Harvard University Press, 1966).

Table 9. Congressmen's Roll Call Positions and Their Constituents' 1964 Congressional Vote[a]

	Goldwater Voters				Johnson Voters			
Vote for Congress	Republican Cong'man		Democratic Cong'man		Republican Cong'man		Democratic Cong'man	
	Conserv. N=61	Liberal N=53	Conserv. N=51	Liberal N=31	Conserv. N=89	Liberal N=99	Conserv. N=123	Liberal N=82
Republican	87%	91%	78%	73%	20%	28%	9%	7%
Democratic	13	9	22	27	80	72	91	93
	100%	100%	100%	100%	100%	100%	100%	100%

[a] Only northern respondents who voted in congressional elections with incumbent candidates are analyzed in this table. The cutting point for classifying Republican Congressmen as conservative or liberal is the mean Republican position on the Administration Opposition index. The cutting point for Democrats is the mean Democratic position on the Liberal Issue index.

Results of the Survey Analysis

Because extraneous causes of congressional voting are canceled out by the "grouping" process, analysis of aggregate data provides a more powerful test for the roll call effect than analysis of survey data allows.[25] But one aspect of the roll call effect can adequately be probed only by analyzing survey data: this is the question of which types of voters are most likely to be influenced by their Congressman's roll call position. A secondary analysis of the SRC 1964 Election data was made in an attempt to answer this question. Intuitively, one would suspect that those voters who are most responsive to the liberalism or conservatism of their Congressman would tend to be the most highly informed—both about their Congressman and about politics generally; one would also expect them to be the most cross-pressured, since an ambivalent choice situation should increase the likelihood that the Congressman's image would tip the balance. As shown below, these expectations receive considerable support from the data.

Table 9 shows the relationship between the Congressman's roll call position and the respondent's congressional vote with the respondent's 1964 presidential choice held constant. For this analysis, the roll call index is a simple dichotomy between each party's relative conservatives and relative liberals, based on the cutting points of the mean Administration Opposition score (for Republicans) and the mean Liberal Index score (for Democrats). As can be readily seen, the differences in the voting rates for liberals and conservatives are quite small. For voters with Democratic Congressmen the small differences are actually in the unexpected direction, as both the Goldwater voters and Johnson voters in the sample gave slightly higher support to liberal Democrats than to conservative Democrats. For voters with Republican Congressmen, the predicted tendency toward more frequent support for liberal incumbents than conservative incumbents receives some support. Among Goldwater voters, who would not be expected to reject a Republican Congressman because of his conservatism, the difference is a small 4 per cent. For Johnson voters the difference is 8 per cent (20 per cent support for conservative Republicans vs. 28 per cent support for liberal Republicans). Although this difference is not very large—and by itself is inconclusive evidence of a roll call effect—it is not inconsistent with the roll call effect estimated in the aggregate analysis.[26]

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[25] Correlations will be larger with larger units (e.g., districts rather than individuals) because through the use of larger units, various disturbing influences are controlled. See Hubert M. Blalock, Jr., *Causal Inferences in Non-Experimental Research* (Chapel Hill: University of North Carolina Press, 1964). pp. 95–114.

[26] In a supplementary exercise, the Congressman's Administration Opposition score was included as an independent variable in a regression analysis of the individual data predicting the vote of the respondents who were Johnson voters with Republican Congressmen. Control variables were the respondent's party identification, the ballot form, an index of the respondent's issue position based on five domestic policy items, and the district two-party vote for President (to control for a slight effect of one-party dominance on congressional voting). The slope for the Congressman's roll call position (−4.9) indicated that an increase of one standard deviation in the Congressman's conservatism decreased the probability of a Johnson supporter's voting for him by 4.9 per cent. A similar regression analysis for Goldwater voters with Republican Congressmen produced the slightly positive slope of +.0.4. Taking into account the fact that 60 per cent of the survey's congressional election voters in districts with Republican Congressmen reported that

Table 10. Republican Congressmen's Roll Call Positions and the 1964 Congressional Vote of Johnson Voters, Controlling for Voter Recognition of the Congressmen[a]

Vote for Congress	Level of Recognition					
	Low		Medium		High	
	Conserv. Cong'man N=29	Liberal Cong'man N=29	Conserv. Cong'man N=34	Liberal Cong'man N=31	Conserv. Cong'man N=26	Liberal Cong'man N=39
Republican	7%	10%	29%	23%	23%	46%
Democratic	93	90	71	77	77	54
	100%	100%	100%	100%	100%	100%
	phi = −.06		phi = +.08		phi = −.18	

[a] The cutting point for classifying Republication Congressmen as conservative or liberal is the mean Republican position on the Administration Opposition Index. The construction of the recognition index is discussed in the text.

When Johnson voters with Republican Congressmen are divided into selected subsamples, the voters who appear to be influenced by congressional roll call positions can be isolated. In Table 10 Johnson voters with Republican Congressmen are divided into three groups on the basis of their degree of recognition of their Congressman. The "high recognition" group (comprising about one third) was able to correctly name the Republican candidate, identify him as the incumbent, and claimed to have read or heard something about him. Voters in the "medium recognition" group passed only two of these three tests, while the remaining "low recognition" group passed no more than one of them. As the table shows, while voters with low or medium recognition did not appear to have discriminated between relatively conservative and relatively liberal Republican Congressmen, a fairly clear difference in the level of support for conservatives and liberals is found for the "high" group. In fact, liberal Republicans received double the support that their more conservative counterparts did (46 per cent vs. 23 per cent) from Johnson voters with the highest recognition of them. Thus, as one would expect, it appears to be necessary for the voter to be somewhat familiar with his Congressman in order to be influenced by his roll call position.

Further divisions of the Johnson voters with high recognition of their Republican Congressmen into additional subsamples reveals that two additional prerequisites for roll call influence (besides high recognition) appear to be high general political information and partisan crosspressures. The relevant findings are presented in Table 11. The level of general political information (high or low) was measured by the test of whether or not the respondent both recognized the Republicans as the more "conservative" party and claimed to have monitored at least three of the four major mass media (radio, T.V., newspapers, and magazines) during the campaign.[27] Relatively crosspressured voters were identified as those Johnson voters with either a Republican or Independent party identification. (The cutting point between liberals and conservatives in Table 11 is slightly adjusted to the median Administration Opposition score for the high recognition group's Congressmen in order to equalize the two categories as much as possible for the small subsamples of voters.) For both the "high general political information" and "crosspressured" groups there is a fairly strong negative relationship between the Congressman's conservatism and the respondent's propensity to vote for him

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they voted for Johnson, the overall estimate of the roll call effect for Republican Congressmen derived from the regression analysis of survey data is a loss of 2.8 per cent of the vote for each standard deviation of conservatism, or:

$$(.60 \times -4.9) + (.40 \times 0.4) = -2.8$$

Given that the partial correlation coefficients accompanying the two slopes are a microscopic −.10 and +.01, this estimate is surprisingly close to the estimates of the 1964 roll call effect for Republicans from the aggregate analysis.

[27] General political information is, as one would expect, correlated with recognition of the Congressman. In the sample of Johnson supporters with Republican Congressmen, 75 per cent of the "high recognition" group but only 47 per cent of the remainder could identify the Republican party as the most "conservative." Sixty-eight per cent of the "high recognition" group but only 55 per cent of the remainder monitored at least three types of mass media during the campaign.

Table 11. Republican Congressmen's Roll Call Positions and the 1964 Congressional Vote of Johnson Voters with High Recognition of their Congressman, Controlling for General Political Information and Party Identification

Roll Call Position of the Republican Congressman[a]	Per cent Voting Republican for Congress					
	General Political Information[b]		Party Identification		Ind. or Repub., with High Info.	Democrat and/or Low Information
	High	Low	Ind. or Repub.	Demo.		
Conservative	17% (18)	40% (20)	38% (16)	23% (22)	17% (6)	31% (32)
Liberal	56% (16)	36% (11)	82% (11)	25% (16)	89% (9)	28% (18)
phi =	−.40	+.04	−.40	−.03	−.72	+.04

[a] The cutting point for classifying Republican Congressmen as conservative or liberal is the median Administration Opposition score for the respondents with high recognition of their Congressman.
[b] The construction of the general political information index is discussed in the text.

(*phi* in each case equals −.40). In contrast, virtually no relationships are found for either the low information or low crosspressure groups.

When the information and crosspressure criteria are combined to produce a small subsample of "Johnson voters with high recognition of their Republican Congressman plus high general political information and crosspressures," the relationship between the Congressman's position with the respondent's vote that emerges is surprisingly strong. Only one of the six voters in this subsample whose Republican Congressman was relatively conservative voted Republican for Congress, while *all but one* of the nine voters within this subsample whose Republican Congressman was relatively moderate or liberal voted Republican for Congress.[28] In sum, the congressional vote of all but two of the fifteen respondents with the common attributes of high recognition, high information, and high crosspressures could be successfully "predicted" on the basis of whether or not their Republican Congressman's degree of conservatism was above or below the median for the high recognition group. This contrasts sharply with the lack of a correlation for the counterpart high recognition subsample with low information and/or low crosspressures. Thus it would appear that virtually the entire roll call effect can be accounted for by the apparent strong susceptibility to roll call influence of the small subsample of voters who, besides recognizing their Congressman, are the most politically informed and crosspressured.[29]

Although the small sample sizes necessitate that these findings be taken with caution, they are sufficiently provocative to warrant some speculation. For example, does the importance of the information factor indicate that the electoral effect of Republican roll call positions is attributable to the electoral behavior of a small but significant group of voters who directly monitor congressional performances? Or do the findings of the survey analysis lend themselves to the less radical explanation that the roll call effect is mainly a function of the two-step flow of communication? Certainly the data offer no firm grounds for rejecting the latter, more plausible, explanation. If the voter needs a minimal amount of information about the Republican Congressman to be influenced by his roll call behavior, it may be that he simply hears "bad" things about extreme conservatives but "good" things about moderates. For these messages to affect the voter's electoral decision, he apparently must be in an initially crosspressured state (such as that of Johnson voters who were not Democratic identifiers) and be highly politically motivated (e.g., high on the political information index). If this interpretation is correct, a sometimes crucial determinant of the Congressman's vote margin is a strong attitudinal consensus that develops among the constituency's relatively informed opinion leaders and followers that, for Republican Congressmen, can be directly traced to the degree of moderation exhibited in roll call performances.

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[28] Even with only fifteen cases, this result is sufficiently strong to be statistically significant at the .05 level, based on Fisher's exact probability test. The subsample appears to be reasonably "representative," since the fifteen respondents are drawn from eleven separate constituencies.

[29] A rough estimate of the proportion of the voting electorate that possesses sufficiently high levels of recognition and involvement to be influenced by the roll call performance of their Republican Congressman, given sufficient crosspressures, is eighteen or nineteen per cent. Eighteen per cent is the proportion of Johnson voters voting in congressional elections with Republican Congressmen who were in both the "high recognition" and "high information" category. Nineteen per cent is the proportion of Republicans and Independents for Johnson who scored high on both recognition and information. These figures, even if somewhat exaggerated, contrast sharply with the reported proportion of voters who in open-ended questions offer appraisals of their Congressman's policy position. Stokes and Miller (*op. cit.*, p. 543) report that "by the most reasonable count, references to current legislative issues comprised not more than a thirtieth part of what the constituents had to say about their Congressmen."

Conclusion

This paper has tested the hypothesis that constituency voting behavior in congressional elections is affected by the degree to which the incumbent candidate's roll call behavior reflects his party's ideological extreme. Although the evidence is inconsistent for Democratic Congressmen, strong support was found for the hypothesis that the degree of conservatism of Republican Congressmen has a considerable negative effect on their vote margins. That constituency voting behavior is affected by the Congressman's roll call performance does not, of course, deductively flow from what is known about the capabilities of the average American voter. The voting behavior literature stresses, quite properly, the lack of political sophistication and awareness of the American electorate. Especially in congressional campaigns, it is known that few voters are aware of the candidates' names and fewer still have any awareness of the candidates' policy positions. From such findings it is all too easy to arrive at the premature and erroneous conclusion that the behavior of candidates has no bearing on the results of elections. Quite clearly—but also mysteriously—the small portion of the voters who are most politically attentive are sufficiently influenced by what candidates do and say to have a measurable impact on election outcomes.

The Development of Policy Thinking in Adolescence*

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I. The Problem

By now, students of political socialization have investigated the development of many political orientations. Among these are attitudes towards authority figures,[1] law,[2] and community,[3] each of which orientations affects the fate of entire political systems, as well as the viability of particular governments. Research has also focused on the evolution of attitudes toward more transient objects, such as political parties and issues.[4] All these forms of research share a preoccupation with preferences and feelings; investigators rarely consider modes of political perception and cognition.

The present paper attempts to remedy this deficiency by extending the study of socialization to include a description of adolescent policy thinking. My effort is directed less toward mapping adolescent political preferences and more toward an appreciation of the styles of cognition and evaluation that adolescents employ when they confront the policy problems of politics. My concern with adolescence proceeds from a belief that articulate modes of political evaluation and cognition do not reveal themselves until childhood is surmounted; hence, adolescence may be a crucial period for the development of policy thinking. Basic orientations and identifications may develop during childhood, but styles of thought are more likely to crystallize during adolescence.[5]

Why should we be interested in how adolescents think about policy problems? For one thing, policy is the very stuff of regime-functioning. How people conceptualize policy problems helps determine the fate of both problem and policy. In turn, the public's understanding of policy problems can either aid or damage a regime's capacity to cope with important social issues. For example, politicians committed to ameliorating a problem must appeal to the public for support, but the quality of public response will be no better than the cognitive and evaluational capabilities people can apply to the policy problem. It is also reasonable to suppose that the closer the public's modes of policy thought are to those of its leaders, the more efficiently will the political system operate.

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[1] David Easton and Jack Dennis, *Children in the Political System* (New York: McGraw-Hill, 1969); Fred I. Greenstein, *Children and Politics* (New Haven: Yale University Press, 1965); Robert D. Hess and David Easton, "The Child's Changing Image of the President," *Public Opinion Quarterly*, 24 (1960), 632–44; Dean Jaros, "Children's Orientations toward the President: Some Additional Theoretical Considerations and Data," *Journal of Politics*, 29 (May, 1967), 368–88; Dean Jaros, Herbert Hirsch, and Frederick J. Fleron, Jr., "The Malevolent Leader: Political Socialization in an American Subculture," *American Political Science Review*, 62, #2 (1968), 564–75; and Edward S. Greenberg, "Political Socialization to Support of the System: A Comparison of Black and White Children," unpublished Ph.D. dissertation, University of Wisconsin, 1969.

[2] Joseph Adelson, Bernard Green, and Robert O'Neil, "Growth of the Idea of Law in Adolescence," *Developmental Psychology*, 1, 4 (1969), 327–32.

[3] Joseph Adelson and Robert O'Neil, "The Growth of Political Ideas in Adolescence: The Sense of Community," *Journal of Personality and Social Psychology*, 6, 2 (1966), 295–306.

[4] Kenneth P. Langton, *Political Socialization* (New York: Oxford University Press, 1969); M. Kent Jennings and Richard G. Niemi, "The Transmission of Political Values from Parent to Child," *American Political Science Review*, 62 (September, 1968), 169–84; Herbert Hyman, *Political Socialization* (Glencoe: The Free Press, 1959); Greenstein, *Children and Politics;* Angus Campbell, Philip E. Converse, Warren Miller, and Donald Stokes, *The American Voter* (New York: Wiley, 1960), Chap. 7; Jack Dennis and Donald J. McCrone, "Preadult Development of Political Party Identification in Western Democracies," *Comparative Political Studies*, 3 (July, 1970), 243–64.

[5] For a psychiatric investigation of thought styles, see David Shapiro, *Neurotic Styles* (New York: Basic Books, 1964). For studies that bear on modes of policy thought, see Robert E. Lane, *Political Thinking and Consciousness* (Chicago: Markham, 1970); Philip E. Converse, "The Nature of Belief Systems in Mass Publics," in David E. Apter, ed., *Ideology and Discontent* (New York: The Free Press of Glencoe, 1964), pp. 206–62; Richard M. Merelman, "The Development of Political Ideology: A Framework for the Analysis of Political Socialization," *American Political Science Review*, 63 (September, 1969), 75–93; and Norma Haan, M. Brewster Smith, and Jeanne Block, "Moral Reasoning of Young Adults: Political-Social Behavior, Family Background, and Personality Correlates," *Journal of Personality and Social Psychology*, v. 10 (1968), 183–201.

Modes of political conceptualization thus affect the political system[6] in many ways.

Policy thinking also influences the quality of a citizen's political participation. A person's capacity to cognize policy may place an upper limit on his sense of political competence,[7] for confidence springs only from a sense of mastery. In addition, capable policy thinking may constitute the foundation of a political ideology.[8] Thus, mature policy thinking may be a waystation on the road to rational, effective political participation. In sum, because policy thinking may be related to the quality of adult political participation and also because the cognitive skills involved in policy thinking first reveal themselves in and are undoubtedly influenced by adolescence, the development of policy thought during adolescence deserves investigation.

Of course, I am interested not only in describing but also in the harder task of explaining policy thinking during adolescence. A couple of hypotheses from the political socialization literature compete for attention as explanatory devices, and in the latter stages of this article I shall attempt very tentatively to apply these hypotheses.

The term, "policy thinking," is hardly common coin in the political science literature. Nor does "maturity" of political thinking sit well in the mind. After all, what seems "mature" to one person may seem hopelessly naive to another. Exactly what is it we mean by "policy thinking" and what aspects of policy thinking do we choose to call "mature"?

Policy thinking consists of four basic forms of thought: moral, cause-effect, sociocentric and imaginative. Let us look at each in detail.

1. *Moral thinking* about a policy problem refers to the justifications a person uses to defend his policy preferences. According to Jean Piaget and Lawrence Kohlberg, moral thinking proceeds through three distinct age-related stages. Piaget and Kohlberg differ on the nuances of moral development, but they agree on the broad outlines of the three phases.[9] The earliest stage is marked by justification in terms of an event's external consequences for the self. The young child equates personally pleasurable outcomes with goodness; hedonism and self-interest are his standards of morality.

The second phase of moral development is marked by the emergence of an internalized norm of reciprocity. Concern with "fairness" to others dominates this stage. Actions are judged by the extent to which they embody equal treatment for all. The child realizes that unfairness cannot be tolerated even if it has no adverse personal consequences. This generalized norm of reciprocity constitutes only an intermediate phase of moral thought, however, because ". . . at the heart of reciprocity there is always an element of self-interest."[10] Reciprocity proceeds from the expectation that if one wrongs another one will be wronged in return. Disinterested moral obligation is still absent.

The most advanced stage of moral thought sees the growth of individual moral principles. Not only has the person internalized a norm of reciprocity, but he also adjusts this strict equality of treatment to take account of extenuating circumstances that require equity and compassion. He is not content with the injunction to treat all alike; morality requires special attention to the complex, idiosyncratic problems of individual cases. Charity must leaven equality. Every person deserves consideration however unlikely he would be to retaliate for ill treatment. In this advanced stage of moral thought, sympathy is justified by reference to qualities of humaneness that unite all people. Principled mercy tempers impartial justice.

People at different stages of moral development may well respond to policy appeals in different ways. A person whose sense of moral indignation is engaged only when his self-interest is threatened will probably be unmoved by references to higher universalistic moral principles. Politicians whose advocacy of a policy rests on a morality their constituents do not share will probably not receive all the support they request. In short, moral thinking about policy—the justifications people offer for their policy preferences—can affect policy formation and implementation.

2. *Cause-effect thought* strives to forge the linkages that hold the world together in consensually valid form. Above all, cause-effect think-

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[6] I derive this line of thought largely from systems analysis. See David Easton, *A Systems Analysis of Political Life* (New York: John Wiley and Sons, 1965), Chap. 7.

[7] For the theoretical underpinnings of a notion of political competence, see M. Brewster Smith, "Competence and Socialization," in *Socialization and Society,* ed. John A. Clausen (Boston: Little, Brown, 1968), pp. 270–321.

[8] For other characteristics, see Converse, "The Nature of Brief Systems . . . ," Campbell, *et al., The American Voter,* Chapter 9.

[9] Jean Piaget, *The Moral Development of the Child* (New York: The Free Press of Glencoe, 1965); Lawrence Kohlberg, "The Development of Children's Orientations Toward a Moral Order: 1. Sequence in the Development of Moral Thought," *Vita Humana,* 6, 1 (1963), 11–33.

[10] Norman J. Bull, *Moral Judgment from Childhood to Adolescence* (London: Routledge and Kegan Paul, 1969), p. 91.

ing is problem-oriented; it recognizes, comprehends, attacks, and, finally, solves puzzles. Evidence suggests that complex problems become increasingly soluble to the maturing adolescent, though intellectual strategies always remain idiosyncratic and occasionally obscure.[11]

I explore two aspects of cause-effect thought about public policy: one is the ability to conceptualize the causes of a problem, and the other is the capacity to visualize the effects of that same problem. Both aspects may be expected to differ from individual to individual and to vary in sophistication as a function of age.

There appear to be three major stages in the development of causal explanations. The earliest stage, confined mainly to young children, involves the indiscriminate listing of explanatory factors.[12] Items on the list are not linked to each other and thus do not add up to a causal chain. The second phase of causal thought sees the formation of linkages between two items. This is a transitional stage on the way to a more advanced form of causal thought, that which involves true causal explanations composed of several links between contributing factors.[13] These three phases constitute the categories I will use for classifying causal thinking about policy problems.

There are at least two main dimensions along which a person may view the effects of a social problem. First, he may choose among the several kinds of people the problem affects. Who suffers from the problem—only those directly involved or other groups in society as well? Is the person aware of the larger ramifications of a problem? I have termed this form of thought personalistic-social effect thinking. Related to this dimension, though distinct from it, is a psychological-behavioral continuum. A person may think mainly in terms of the psychological toll a problem takes on those who suffer from it; or he may connect feelings to socially relevant behavior. I will investigate both of these policy effect thinking continua.

The public's capacity to reason about social cause and social effect may limit its receptivity to different policy rationales. People who cannot understand that political difficulties are produced by and, in turn, affect complex social forces may resist policies meant to unravel the institutional network responsible for these difficulties. Thus, cause-effect thinking, like moral thinking, constrains the appeals that politicians make and the policies they pursue.

3. *Sociocentrism* is the realization that complex social institutions and practices with their own rules create social problems. It presupposes the recognition that political problems cannot be reduced to the motives, intentions, or behavior of particular people. The sociocentric individual, unlike his more personalistic fellows, also understands that only collective action, based on communally defined standards, can solve such problems. I investigate two dimensions of this complex variable. The first is the individual's capacity to explain a social problem by reference to distinctly social rather than individual, psychological, or personal causes. This form of thought complements the social-personalistic dimension of effect-thinking described above. The second is what I call definitional sociocentrism, by which I mean a person's willingness to accord society as a whole some responsibility for defining the extent of a social problem. A truly sociocentric perspective accords special responsibility to the collectivity by recognizing that society cannot be made up of continually warring interest groups each concerned only with its own particular problems.

The presence of sociocentric thought in the public is crucial to policy formation, for without it an individual is ill-prepared to legitimize collective problem solving. A sociocentric frame of mind accords legitimacy to the policymaking efforts of government.

4. *Imaginative thinking* about policy takes at least three forms. The first is hypothetical thought, which is defined as a person's capacity to visualize social changes should a current social problem be expunged. Hypothetical reasoning bespeaks a capacity to link current policy proposals to their consequences in action. Thus, hypothetical thought is a prerequisite to full comprehension of policy proposals.

I term a second form of imaginative thought role-taking, which I define as a person's capacity to invent arguments contrary to his own policy preferences. Role-taking capacity not only strengthens comprehension of a policy problem, but also brings proponents and opponents of particular policies together in imaginary dialogue, thus cooling the heat of political conflict and smoothing the course of policy making.

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[11] For an examination of the development of causal thought see Irvin E. Sigel, "The Attainment of Concepts," in M. L. and L. W. Hoffman, eds., *Review of Child Development Research*, Vol. 1 (New York: Russell Sage Foundation, 1964), 209–49. On the idiosyncratic aspects of problem solving, see Guy T. Busell, "Patterns of Thinking in Solving Problems," *University of California Publications in Education*, V. 12, 2 (1956), 63–148.

[12] David Elkind, "Cognitive Development in Adolescence," in James F. Adams, ed., *Understanding Adolescence* (Boston: Allyn and Bacon, 1968), pp. 128–59, 141–2.

[13] This conceptualization derives largely from Bärbel nhelder and Jean Piaget, *The Growth of Logical Thinking from Childhood to Adolescence* (New York: Basic Books, 1958).

The last aspect of imaginative policy thinking we will explore is linkage thought, which is the ability to see the consequences to one's self of social problems that, in the first instance, plague others. Linkage thought obviously engages creative skills not usually employed in constructing straightforward causal explanations of closely related phenomena. Visualizing the personal effects of seemingly distant issues lends emotional immediacy to a political problem and produces a climate receptive to proposed solutions. Linkage thinking thereby provides a motivational foundation for policy innovation.

To sum up: I investigate four forms of policy thought with nine variants. The following chart summarizes the components of policy thought:[14]

The Components of Policy Thinking[15]

Dimension	*Variants and Description*
Moral Thought	No variants. The capacity to justify one's policy preferences by reference to universalistic norms of reciprocity or individual moral principles rather than by hedonistic calculation of external effects.
Cause-Effect Thought	Causal thinking: The capacity to produce explanatory chains for social problems.
	Effect Thinking: Social vs. Personalistic: The capacity to see general social ramifications of social problems.
	Effect Thinking: Psychological vs. Behavioral: The capacity to visualize behavioral effects of policy problems, rather than simply attitudinal effects.
Sociocentrism	Social Source Thinking: The capacity to locate the source of a political problem in distinctly social institutions rather than in personal predilections or frailties.
	Definitional Sociocentrism: The willingness to allocate a collective entity—government—the right to define the extent of a social problem.
Imaginative Thinking	Hypothetical Thought: The capacity to *imagine* the effects of removing a social problem. This ability involves reasoning about a contrary-to-fact or hypothetical situation, i.e., the world minus the social problem at hand.
	Role-taking: The capacity to construct arguments contrary to one's preferences.
	Linkage Thought: The capacity to see the effects of a distant social problem on the circumstances of one's life.

Scattered evidence from the literature of developmental psychology leads us to expect these forms of thought to mature as a function of age.[16] Of course, psychologists have rarely investigated modes of political conceptualization. Therefore, we should not be surprised if developmental psychology turns out to be a somewhat fallible guide to understanding policy thought during adolescence.

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[14] Parenthetically, students of public policy rarely consider in any detail the varying methods by which the public perceives policy. There is no examination at all of this problem in two recent and influential attempts to synthesize the emerging field of policy analysis. See Austin Ranney, ed., *Political Science and Public Policy* (Chicago: Markham, 1968); and Ira Sharkansky, ed., *Policy Analysis in Political Science* (Chicago: Markham, 1970).

[15] There is probably more to these dimensions than just cognition. To some uncertain extent successful performance may be facilitated by a sympathetic identification with the poor. This is especially apparent for moral, empathic, and linkage thought, but also applies to the other dimensions in lesser degree. That motivation supports these forms of cognition does not detract from their cognitive status, however, for they remain forms of cognition. Moreover, identification may grow from comprehension just as comprehension may grow from identification. Put another way, motivation is always based partly upon cognitive attainments as well as upon affect.

This comment raises a seemingly more important general question. Can we really treat these dimensions as if they are independent of particular political preferences? Certainly many of the scales appear to chart development from a set of perspectives associated mainly with the Right to a set associated mainly with the Left. Might there be some normative bias associated with the definition of dimensions and the construction of scales? Perhaps, but most of the developmental literature from which the dimensions were drawn provides *empirical* support for age-graded development in the directions indicated by the discussion. At best, therefore, an argument might be made to the effect that a different set of dimensions might yield a pattern of development from apparently Left to apparently Right political perspectives. But such an argument need not deter our investigation.

[16] On sociocentrism, see especially Jean Piaget, *Th Language and Thought of the Child,* trans. Marjori Gabain (Cleveland and New York: Meridian Books 1967). For hints about the development of orienta tions relevant to policy thinking, see also: Marian Radke Yarrow and John D. Campbell, "Person Per ception in Children," in Ira J. Gordon, ed., *Huma Development* (Chicago: Scott, Foresman, 1965), pp

II. The Study

The study took place during late June and early July, 1970, in a middle-class suburb of Madison, Wisconsin. School officials cooperated in the selection of twenty high school seniors and twenty eighth-graders. The latter were all aged 13 and 14, the former 17 and 18. Only "A" and "B" students were included in the sample as a rough control on intelligence and academic motivation. The final sample consisted of 19 "A" and "B" seniors and 18 "A" and "B" eighth-graders.

The author personally conducted tape-recorded depth interviews with each member of the sample. Most meetings occurred privately in the student's home, a few in the author's office. Subjects were informed on first telephone contact that the author was interested in young people's views on "a political problem." To prevent contamination, I made no mention of the particular policy problem that was the subject of the conversation until the interview began. Each respondent was assured anonymity, and the author pointed out that no student's full name could be found on the tape.

Poverty was chosen as the subject of the interview. Poverty suggested itself because, for one thing, it engages our perceptions of justice, law, and personal obligation. The problem of poverty has always occupied a unique moral position in both political and theological debate. Moreover, the causes of poverty, especially the "vicious circle,"[17] are complex and socially centered. They therefore lend themselves easily to an examination of causal thinking. In addition, although the effects of poverty are substantial, they nonetheless challenge the reasoning capacities of middle-class people, most of whom are far removed geographically from poverty-stricken areas. Poverty thus provides a good opportunity to examine the modalities of effect thinking. Moreover, the widespread view that the poor are lazy and irresponsible competes with a more institutional explanation of poverty's origins. Hence, poverty is a spy glass through which to examine the growth of sociocentric conceptualization. Finally, poverty is a familiar enough social condition to provide ample opportunity for imaginative intellectualization. A more abstruse issue that did not evoke images of a particular subgroup would have stimulated the imagination less. To sum up: the problem of poverty promised to tap the sorts of thinking that appeared central to this investigation.

The sample included equal numbers of boys and girls. Analysis for sex differences turned up nothing of either statistical or substantive importance, and, consequently, I do not report findings by sex. The interviews averaged about forty minutes, mostly taken up with the discussion of poverty. Most of the interviews took place in an atmosphere of repose and rapport. The few problems of resistance were confined mainly to eighth graders.

The reader should keep in mind the problem of treating cross-sectional findings as if they were truly developmental. Given the youth of the respondents and the political turbulence of the last few years, it is easily conceivable that the sampled eighth graders were very different from the eighth graders of four years ago. In addition, the small sample size admonishes us to proceed with special caution, a caution reinforced by the fact that coding open-ended questions is a sensitive procedure. The investigator and his research assistant each categorized responses independently, and when agreement on a dimension's scores surpassed the .80 level (utilizing the Pearson product-moment correlation), which it frequently did, we deemed the results acceptable. When the coders could not meet this standard of reliability initially they refined their understanding of categories and consulted together until agreement did rise to .80. Influence in such consultations ran in both directions; the final codes and scores are a genuine amalgam. As we review our findings I will describe in detail the questions used to tap particular dimensions, together with the codes and categories employed. I will also provide representative examples from selected interviews.

a. Moral Thought. Moral reasoning was assessed along a five-point scale based on the three phases of morality described by Piaget and Kohlberg outlined above. Scores came from responses to one general question and two probes. The investigator asked each student whether or not he thought anything could be done about poverty. After he answered, the investigator asked him whether anything *should*

265–76; Herman A. Witkin, Donald R. Goodenough, and Stephen A. Karp, "Stability of Cognitive Style from Childhood to Young Adulthood," *Journal of Personality and Social Psychology*, 7 (October, 1967), 291–301; Haan, Smith, and Block, "Moral Reasoning . . ."; Elkind, "Cognitive Development . . . ," and Eleanor E. Maccoby, "The Development of Moral Values and Behavior in Childhood," in Clausen, ed., *Socialization and Society*, pp. 227–70.

[17] For a comprehensive picture of the vicious circle at work, see Kenneth B. Clark, *Dark Ghetto: Dilemmas of Social Power* (New York: Harper and Row, 1965), Chaps. 3 and 6.

be done about poverty. The usual reply was that poverty should be eliminated if possible; the investigator then asked the respondent why he felt as he did, probing in the absence of clear findings from this general question. Some subjects (such as P. below) appeared to waver between different levels of moral thought. To probe their thinking further, they were asked to explain and justify what they would do if a legitimate and popularly supported American regime attempted through normal legislative channels to prevent people from aiding the poor. Additionally, in order to separate respondents at the level of reciprocity from those evidencing a morality of individual principle, the author occasionally inquired of a student what he would favor if, although a government program assured the poor equal treatment, the poor still lagged behind others in society. We coded higher those who declared their continued readiness to aid the poor even though the norms of equality and reciprocity had been met. Such respondents had clearly gone beyond the strict norm of reciprocity into a morality of principled compassion. The variety and complexity of moral thinking required us to expand the three phases of morality into a five-point scale. Scores thus ranged between a low of one and high of five, according to the following criteria: 1—justification of poverty preferences by reference to the material consequences of poverty; 2—justification by reference both to poverty's material effects and to a norm of equal treatment for the poor; 3—justification by reference to the norms of equality; 4—justification by a combination of reciprocity and an appeal to individual moral principles, equity, ethical commitment, compassion, or conscience; 5—justification solely by reference to individual moral principles.

Table 1 presents our findings:

Table 1. Age Differences in Moral Thinking About Poverty

(mean scores on scale of 1–5)

Grade Level	Mean	Index (1–100)[a]
Eighth	2.50 (18)	37½
Twelfth	3.63 (19)	62
P<.001		

[a] The index figures convert the means to standard positions on a percentile scale in order to facilitate comparison of one form of thought with another. Such comparison would otherwise be hindered by the variable number of categories from dimension to dimension.

Let us examine two very different kinds of moral thinking about poverty:

Investigator: Some people feel that we should try to do away with poverty. Other people feel that a certain amount of poverty is a good thing. How do you feel? Do you think we ought to reduce poverty or not?

P: (boy, 18) I consider it necessary . . . I think that as long you've got some people that are under; we consider this country supposedly to be the land of equals and when some people are underneath or are being discriminated against because they're poor, I think that basically it's something that's necessary to try to bring them up.

Investigator: Why is it necessary to do that, do you think? You are attributing quite a bit to the beliefs the country has about inequality. What about your own feelings? What do you think? What do you think is wrong about it necessarily?

P: Well, just that they're not able to achieve, yeh, to achieve everything that other people are able to achieve because they're poor. They're not able to do the same things.

Investigator: They just don't have as good a chance?

P: Right.

Investigator: Suppose that we had a situation in which we arranged it that all people had an equal opportunity to achieve. And we still had large numbers of people who remained poor because they just didn't take advantage of the opportunity to rise. Then what would your feelings be about that? Do you think that you would help those people, too?

P: You'd have to because these are really the hard-core poor, the people who have been in this cycle for generations. They're really so depressed in spirit that they're really not able to help themselves because they really don't see any potential for help . . .

Investigator: You say they can't do it for themselves, but why should we do it for them?

P: They deserve at least a chance. They're still human . . . If we believe that humans should have the same rights, they should be given these rights; they should be taught how to use them.

P. is a hybrid. He oscillates between the language of reciprocity and the internalization of compassion, therefore falling into our fourth category. His thinking contrasts vividly with the spare views of J., an eighth-grade girl:

Investigator: Do you think we ought to try and get rid of poverty? Some people feel we should; other people feel we shouldn't. What do you think? Do you think we ought to or not?

J: Yes.

Investigator: Why do you think so.

J: Cause I think it solves our problems.

Investigator: What sorts of problems do you have in mind?

J: The usual problems.

J. falls rather convincingly in the lower reaches of moral thinking about poverty. She judges almost wholly in terms of the instrumental consequences of poverty for society.

As Table 1 indicates, in our small sample there appears to be a marked advance toward a fully internalized policy morality during adolescence. Still, the twelfth-grade mean falls only slightly beyond the morality of reciprocity and equality.

b. Cause-Effect Thinking: 1. Causal Thought. One common scholarly explanation of poverty is the "vicious circle," a concept that joins together the cumulative effects of previous poverty, inferior housing, race, ill-health, low education, and low income, all acting in concert to perpetuate poverty. Judging explanations of poverty in terms of the vicious circle constitutes a convenient measure of causal thinking about poverty. We defined a vicious circle explanation as the coupling of at least three causal factors in a two-link sequence. Less sophisticated respondents linked just two causes, simply listed several unrelated causes in no apparent order, or mentioned only single causes. We arranged these responses on a scale of 1–4, with the vicious circle explanation coded 4, single causes 1, multiple unrelated causes 2, and two linked causes 3.[18] Table 2 reports our findings:

Table 2. Age Differences in Causal Thinking about Poverty

(means on scale of 1–4)

Grade Level	Mean	Index (1–100)
Eighth	3.00 (18)	67
Twelfth	3.21 (19)	74

not significant

Table 2 reports only slight, statistically insignificant movement toward causal sophistication between eighth and twelfth grades. Most eighth graders in our sample could link together two causes of poverty, a performance upon which their older schoolmates improved little. The following excerpts from our interviews, therefore, are offered only as random but particularly vivid examples of the variety of explanations our respondents proffered, and are not meant to imply any developmental trend.

Interviewer: What are the things that make people poor, do you think?

D: (girl, 18) Well, in our society it might have something to do with the rich getting richer and the poor get poorer. If you're poor, you can't get an education, and if you don't have an education, you can hardly get a good job. Education leads to a good job.

This vicious circle explanation contrasts vividly with the rather fragmented response to the same question by M., an eighth-grade boy:

One way could be the people themselves. They just don't want to work. And others—they just have a run of bad luck or something like that. If they were working on stocks, they just go up and down all the time and they lose.

M. supplies us with only a laundry list of personal frailties and mishaps;[19] there are no links to hold his explanatory factors together, and he fails to mention even one purely social item.

2. Effect Thinking: Social vs. Personalistic. The author investigated a respondent's capacity to visualize poverty's social effects by simply asking, "What sorts of effects does poverty have?" We scored at level one those who focussed solely on the poor. Others, whose main concern fell on the social ramifications of poverty, were placed at level two. We reserved level three for those who welded personal and social effects together in their explanations. Table 3 reports our findings:

Table 3. Age Differences in Poverty Effect Thinking: Social vs. Personalistic

(means on scale of 1–3)

Grade Level	Mean	Index (1–100)
Eighth	1.80 (18)	40
Twelfth	1.76 (19)	36

not significant

Table Three reveals no relationship at all between age and a social conception of poverty's effects. This finding became our first hint of highly selective development in policy thinking. Let us now illustrate some contrasts in social-personalistic effect thinking:

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[18] This "vicious circle scale" places a premium on the ability to see interrelationships rather than on the capacity to approach poverty from multiple perspectives. My belief is that inferring causality is a matter of constructing connections and, ultimately, syntheses. Other approaches to poverty might be explanatory, but they do not highlight causal thought.

[19] This concentration on the vagaries of fortune is reminiscent of the "magical thinking" very young children manifest. For examples, see Jean Piaget, *Play, Dreams and Imitation in Childhood,* trans. C. Gattegno and F. M. Hodgson (New York: W. W. Norton, 1962, first published, 1951), pp. 257–8.

B: (boy, 18) (Poverty) is a problem because, with poverty I think . . . especially with children poverty brings poor education, poor diets, and this all adds up and you get a lot of people who just aren't . . . we, I consider them human beings, I mean they are there, but they really had so much potential, they could have been reached at an earlier age and been better developed.
Interviewer: So it certainly has a deteriorating effect on the poor people themselves. Can you think of any other effects that poverty has besides that. Does it have any effect on the country in general or the people of the U.S., do you think?
B: No. Not especially because I think the people just put it in the back of their minds and say it doesn't exist . . .

B's answer reflects a characteristic tendency in our respondents, namely to believe that those who can ignore problems escape their effects. Even when the interviewer specifically directed him towards social effects in order to test the provincialism of his thinking, B. could not move forward. His naivete contrasts markedly with the perspective of S., an eighth-grade girl:

Interviewer: Can you see any effects that poverty has on the U.S., on the country, or on the people or anything like that? Does it have any effects you can see?
S: Um, well, like I think it might be in voting. When they vote for President and stuff.
Interviewer: How so?
S: The people aren't educated so they don't really know much about the voting and how to choose a right President or Senator or Congressman. And so when they vote they sort of just say, 'Oh, that looks like a good one so I'll pick him.'
Interviewer: Any other effects beyond that?
S: Well, um, in strikes. Many times people who are under-educated and in poverty don't know what they're marching or rioting for.

We need not accept S's diagnosis in order to recognize her sensitivity to the social ramifications of poverty. The consequences she describes pertain initially to the political activities of the poor, but these activities are of a sort to affect other groups in society as well.

3. Effect Thinking: Psychological-Behavioral. We used the question just described as a means of assessing the psychological-behavioral dimension of effect thinking. To what extent can adolescents understand that poverty affects interpersonal behavior as well as intra-individual feelings that have little political impact? We coded respondents who mentioned only the psychological effects of poverty at level 1. Those who perceived only behavioral effects we placed at level 2, while those who described both kinds of effects we elevated to level 3. Table 4 reports our findings:

Table 4. Age Differences in Poverty Effect Thinking: Psychological vs. Behavioral

(means on scale of 1–3)

Grade Level	Mean	Index (1–100)
Eighth	2.29 (18)	65
Twelfth	2.06 (19)	53

not significant

Again we discover that genetic maturation is apparently no guarantee of mature policy thinking. Many students, whatever their age, could visualize the behavioral effects of poverty; few linked those consequences, however, to their psychological concomitants. The following excerpts illustrate contrasting approaches to the problem.

Interviewer: Do you think poverty is an important problem in the U.S.?
M.: (girl, 18) Yeh, I do. I think that everyone has a problem, you know, like but it's just as important as everything else.
Interviewer: When you say it's just as important as everything else, is it more important than some things? Or less important?
M: I don't know. Right now to me it's . . . one of the most important things is the environment and the population problem because, maybe even the population problem is the most important, because if we don't solve that, we're not going to have to worry about anything else. And I thought that had to do with poverty too.
Interviewer: How so?
M: Well, I think that many times people that are poverty-stricken have more children . . .
Interviewer: OK. Can you think of any effects that poverty has generally on the country or on the people? You mentioned the question of overpopulation. Certainly that would be one. Can you think of others?
M: I'm sure that everyone that is in a poverty situation is very dissatisfied with the way that they're living; and I'm sure that they would think that someone should be doing something for them, particularly the government. And they wouldn't be as willing to cooperate with anything the government was willing to do, you know. They would feel even paranoid toward everyone that was better off than they were.

M. describes several psychological and behavioral ramifications of poverty. Certainly there is little disagreement about the major problems overpopulation may produce. M.'s view contrasts vividly with that of D., an eighth-grade girl, who for some reason cannot proceed beyond her vision of poverty's substantial psychological effects.

D: It makes some people mad when they haven't got a lot of money and hate people who do and . . . Like people who live in poor areas, they just don't like people who have more money than they do.

Interviewer: Can you think of any other . . . Yes, go ahead. Can you think of any other effects it might have?

D: Yeh, I don't know.

Despite the investigator's proddings, D. cannot quite make the intellectual leap involved in tying psychological tendencies to behavioral outcomes. Her partial visualization was typical of many respondents.

c. Sociocentrism: 1. Social Source Thinking. The author investigated social source thinking by asking the respondent whether he thought that things about the poor person himself explained his poverty best, or whether things about the environment of poverty, such as the school or the neighborhood, were more important. We coded at level 1 those respondents who chose the personal explanation, at level 2 those who combined the personal with the environmental, and at level 3 those who centered wholly on the more complex and abstract environmental interpretation. Table 5 reports our findings:

Table 5. Age Differences in Social Source Thinking About Poverty

(means on scale of 1–3)

Grade Level	Mean	Index (1–100)
Eighth	2.35 (18)	68
Twelfth	2.34 (19)	67

not significant

Age is unrelated to social source thinking. Most students at both grade levels accepted an environmental explanation of poverty. Still, responses to the question can be quite different, as the following excerpts suggest:

Investigator: Some people think that the things that make people poor are things about the person himself and the kinds of ideas and values he has. Other people think it has to do with, like, the neighborhood a person grows up in or the school he goes to—stuff like that. What do you think is more important?

S: (girl, eighth grade) I think the person himself, because, well, every person has their different standards, moral standards like, and it really depends on the person. Not where they live or how they're brought up or anything. That may have a little effect, but . . .

S. resists connecting the moral propensities of the individual to their environmental sources. Her view stands in sharp contrast to that expressed by J., an alert eighth-grade boy.

Well, the area he grows up in gives him a lot of his personality and the things inside him.

J. is far more discerning than S.; his view typifies most of our respondents at both grade levels.

2. Definitional sociocentrism. I have argued that poverty, which has ramifications far beyond the poor, implies some equal and collective responsibility to define the problem's boundaries. But a still more sophisticated conception accords those suffering from poverty some special opportunity to take part in the definitional process. This opportunity reflects the extension of equity and compassion to the victimized, a tendency encouraged by and paralleling mature moral thought. Thus, we assigned our highest score to those respondents who described boundary definition as a joint undertaking of the poor and society. We accorded a purely collective focus second position, and relegated a wholly nonsociocentric perspective, which places sole definitional responsibility either in the hands of the poor or some other subgroup, to the lowest category. The author operationalized this dimension by asking the respondent who he thought should decide if a person is poor—the poor person himself or some other agency. Table 6 reports our findings:

Table 6. Age Differences in Poverty Definitional Sociocentrism

(means on scale of 1–3)

Grade Level	Mean	Index (1–100)
Eighth	1.86 (18)	43
Twelfth	2.21 (19)	61

$p < .01$

Age significantly accelerated definitional sociocentrism; although eighth graders could not fully accept a definitional role for government, twelfth graders found such a role quite congenial. The following examples suggest the diversity of conceptions along this dimension.

Interviewer: Some people say that the only person who can say whether a person is poor or not is the person himself, 'cause he kind of knows how he's living and what he's making and how he feels. Other people think that it has to be a community group or maybe a government agency which will decide whether a family is poor or not. Who do you think should decide? Should it be the person himself or an outside agency or group or what?

L: (girl, twelfth grade) I think it should be an outside agency because a lot of people, especially the people who live in the mountains and stuff, they have so much pride they wouldn't admit they were poor and they would let their family live in really bad conditions. And they wouldn't think they were poor.

L. buttresses her belief in collective responsibility with the recognition that, left to themselves, many people who require help would refuse it, thereby harming not only themselves but innocent others. Her quite sensitive response may be contrasted with that of R., an eighth-grade boy:

R: I think it should be the people themselves.

Interviewer: Why do you think so?

R: Well they should know. I mean, like they could live in poor houses and that and they could have a lot of money, but it's just that they don't care that much. But I think the people themselves.

Interviewer: I see. Why do you think another group of people perhaps shouldn't decide?

R: Well, it's really not much their business and that. They just, I don't think they could get all the facts on it.

R.'s last remark reveals a lack of confidence in both the morality and efficiency of the community. His jaundiced view was not typical, however.

d. Imaginative Thinking: 1. Hypothetical Thought. The author assessed the respondent's capacity to reason hypothetically by asking him what changes an end to poverty might bring in the United States. Because poverty is currently a severe problem, visualizing the country in its absence requires hypothesizing. Our respondents fell into four categories on this dimension. Least successful were those who could conceive of no differences, a bit more successful were those who could envision attitudinal or motivational effects, even more successful were those who could see important social, political or economic effects, and most successful were those who added attitudinal or motivational effects to social, economic, or political consequences. The definition of categories 2 and 3 is predicated on the personalistic-collectivity continuum already investigated in the sociocentric dimensions. Table 7 reports our findings:

Table 7. Age Differences in Hypothetical Reasoning About Poverty

(means on scale of 1–4)

Grade Level	Mean	Index (1–100)
Eighth	2.83 (18)	60
Twelfth	2.76 (19)	58

not significant

Again, surprisingly, we observe a null relationship between age and a form of policy thinking. Indeed, eighth-graders seemed a shade more capable of imagining an America without poverty than twelfth-graders. The following excerpts illustrate quite different assessments of a poverty-less United States:

Interviewer: Let's imagine for a moment that there were no more poor people in the country—that we had gotten rid of poverty in some way. Can you think of any differences that would make in the country—any changes that would occur if we had no more poverty?

P: (boy, 18) It's difficult to imagine a country like that. It always seems that the poor are a potential source of change in a country and to accommodate them in some way like this would be, you'd have to completely start over . . . You'd need much more sharing that the upper class would be able to share with the poor, and I don't see this country doing this.

Interviewer: Why do you think that's so impossible?

P: Well, basically this is still a capitalistic society and as long as there are some people that would like to keep their wealth—selfish, to put it that way—they'd be unwilling to contribute this way.

P's employment of such abstract concepts as class and capitalism bespeaks a very sophisticated view of society, one atypical among the respondents in our sample.[20] P. couples the removal of poverty to a more general and far-reaching remaking of American life. Contrast P.'s vision with the restricted imagination of E., another twelfth grade boy:

E: I think there always will be poor people no matter what happens. I really don't think that you can say that—this is very theoretical—that you haven't, because you're always going to have people on the top, people in the middle, and people on the bottom...

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[20] The use of such terms, though rare, varies directly with the age of the respondent.

Interviewer: So it's very difficult to think about what changes might occur. It's a hopelessly idealistic kind of thing.
E: Because right now the whole thing you don't hear much about it, and you know, it doesn't seem to affect anybody right now. But if that poverty is wiped out, then you'd have to wait and see what it's like then to see what happens.

E. begins like P. by initially resisting the hypothesis that poverty might end; but when he does finally confront the question he cannot cope with its demands. His ideas are limited by concrete experience, not expanded by intellectual skills.

2. Role-taking. One of the most difficult of all mental performances is viewing the world through the eyes of an opponent. For this reason I doubted that many respondents would be able to construct arguments contrary to their own poverty preferences. The author investigated role-taking capacity by first ascertaining whether a respondent thought anything should be done to eliminate poverty. Almost all agreed that something should be done. I then asked each respondent whether he could imagine any reasons why someone might disagree with him and contend that poverty might, in some ways, be a good thing. Those who could find no such reason we placed at level 1. Those who could only invent arguments that were clearly fallacious, trivial, or insubstantial we relegated to level 2. We placed highest those who offered more substantial and widely supported arguments citing the supposed incentive aspects of poverty or the alleged social need for some body of menials. Table 8 discloses that our minimal expectations about the role-playing capacities of both groups were well-founded.

There was just the barest tendency for older respondents to be more capable of role playing than eighth graders. More important, however, is the fact that most respondents could not invent substantial opposing arguments. Here are examples of less and more successful approaches to the question.

Table 8. Age Differences in Role-taking Reasoning About Poverty

(means on scale of 1–3)

Grade Level	Mean	Index (1–100)
Eighth	2.03 (18)	52
Twelfth	2.13 (19)	57

not significant

Interviewer: Some people feel that a certain amount of poverty is a good thing for a society—that every society needs a certain number of poor people. Other people disagree with that. What's your own feelings? Do you think society can profit from having poor people?
J: (girl, 18) It can profit, but . . .
Interviewer: I don't mean profit necessarily, but . . .
J: Yeh, but I think . . . There should be a few poor people, I suppose, but not as many as we have.
Interviewer: Why do you think there should be a few?
J: Really, no definite reason. It just seems like it would work better that way if there were, it would kind of balance everything out. I have no definite reason. It seems like it's always been that way.

J. conjures up the vision of a traditional social equilibrium disturbed by the abolition of poverty. She is uncertain, however, of the role poverty plays in maintaining this equilibrium, nor can she describe any of the social dynamics involved. Her somewhat muddled response may be contrasted with the clarity of C., a bright, engaging eighth-grade boy:

C: I think the only reason a person would think poverty is necessary is that they needed someone they could rank over . . .

C.'s use of the "rank" concept reveals an incipient, but nonetheless firm grasp of the incentive argument for poverty. C. seems to be saying that poverty fuels the competitive urge many people feel. Few respondents equaled C.'s role-taking sophistication.

3. Linkage Thinking. The last form of policy thought investigated is the capacity to see the personal consequences of distal social problems.[21] The bulk of our respondents came from comfortable middle-class homes; therefore, to them poverty had no direct, visible meaning. Imputing personal resonance to such a problem required, among other things, an effort in imagination. The author asked each student whether he saw any effects that the existence of poverty had on him or his family. Those who could see no effects were coded at level 1, those who saw only psychological or rhetorical effects at level 2,[22] and those who mentioned political, economic, or social effects at level 3. (Of course, recognition of poverty's costs does not preclude, nor is it precluded by, the realization that removing poverty would also be costly). Table 9 reports our findings:

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[21] I have taken the "distal" concept from James C. Davies, who applies it to social groups, and have used it to characterize perceptually distant issues. See James C. Davies, *Human Nature in Politics* (New York: Wiley, 1963), p. 183.

[22] "Rhetorical effects" refers to the considerable number of respondents who said that others' poverty

Table 9. Age Differences in Linkage Thinking About Poverty
(means on scale of 1–3)

Grade Level	Mean	Index (1–100)
Eighth	1.64 (18)	32
Twelfth	1.89 (19)	44

$p < .025$

Our respondents performed worst on this dimension. Most mentioned only a few superficial effects. Age dampened this depressing picture somewhat, but hardly enough to lend much comfort to the advocate of a war against poverty. Let me illustrate two very different responses to the question. First notice how L., a politically alert, earnest, and articulate twelfth-grade girl, struggles with the problem:

Interviewer: Do you think that the existence of poverty in the United States has any effect on your personally or on your family, the way you live or the things you do? Does it have any effect on you, do you think?

L: I know it does. I know it does because—well, it's . . . I don't know, maybe it doesn't, but I think it does because, well, it has to have an effect on me if I don't do anything about it, yet I know it exists. That must have an effect on me as a person.

L. feels disquieting pangs of guilt about poverty, but seems unaware that poverty raises the taxes her family pays or requires them to support welfare programs. By contrast, P., a voluble and occasionally unpredictable twelfth-grade boy, after first referring frivolously to his family's personal tie to poverty, addresses the question quite accurately:

P: My dad is relief director for ————.

Interviewer: For what? (laughter)

P: Trace it! (more laughter)

Interviewer: I see. So your father has . . .

P: Well he's also got a lot of contact with other people, being a minister too.

Interviewer: Sure.

P: And I get in on some of this, but he tries to keep that with his work, you know, and not, and keep other people's problems himself and not talk about it when I'm around, because it's not really any of my business.

Interviewer: Yeh, right. I see.

P: But also seeing part of my pay check go out . . .

When P. finally reflects on the larger dimensions of the problem, he realizes that poverty costs him money. Perhaps his father's unique position aids him in making the proper connection.

To summarize, of nine dimensions of policy thought, age was associated with significant movement in three: moral thinking, linkage thinking, and definitional sociocentrism. With increased age causal thought and role-taking capacity also expanded. But, contrary to what Piaget and others might expect, in this study age was either negatively related or wholly unrelated to both forms of effect thinking, to social source thinking, and to hypothetical thought. The effects of genetic maturation appear to be limited and selective in this study.

III. Discussion

There are two ways in which a pattern of intellectual change during adolescence can be approached. One common interpretation is environmentalist. It argues that changes in policy thought during adolescence can be explained almost entirely by the changing effects of such environmental agencies as social class or the school.[23] A second, less popular explanation takes a developmental perspective. It claims that policy thinking during adolescence is regulated as much by genetic maturation as by social learning.[24]

Neither of these approaches by itself satisfactorily explains our findings. We discover development to be selective; yet the two theories treat development as global. It is clear that only a carefully constructed and refined amalgam of the two approaches will be convincing.

Although most of our discussion must be speculative, we can investigate empirically some considerations related to these two hypotheses. We were in fact able to test a com-

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caused their parents to stress to them the importance of eating all the food they were served. Poverty's sole effect, in these cases, was as a goad to the inculcation of good habits. These superficial effects, hortatory and rhetorical in character, are unlikely to influence more fundamental perceptions of poverty.

[23] Prime examples of the environmental approach are Langton, *Political Socialization* . . . , and Jennings and Niemi, "The Transmission . . ."

[24] Examples of this approach include the articles cited above by Adelson and his colleagues: Merelman, "The Development of Political Ideology . . ." Elliott S. White, "Intelligence and Sense of Political Efficacy in Children," *Journal of Politics*, v. 30 (August, 1968), 710–32; and, to a limited extent, Robert E. Lane, "Political Education in the Midst of Life's Struggles," *Harvard Educational Review*, 38, 3 (1968), 468–95.

mon form of the environmentalist position, the systems analysis approach to socialization, which would predict that the political system itself is a major shaper of policy thinking.[25] Certainly Easton and Dennis' discovery that images of political authority diverge from perceptions of parental authority as the child matures supports such an expectation.[26] Easton and Dennis' finding apparently indicates that as the child ages, the political system gradually becomes for him an autonomous socialization agency, capable of inculcating its own particular brand of attitudes.

A logical correlate of this argument is that a student's policy thinking should vary with his involvement in the political system. A politically responsive student should be particularly receptive to the system's cues, and therefore apt to adopt its usually sophisticated policy rationales. But was individual politicization related to policy thinking? Not really. We dichotomized our sample into high and low politicization groups on the basis of several questions asked at the end of our interview.[27] Differences in policy thought between low and high politicization types were weaker than those between younger and older students. Highly politicized students were usually more advanced in their policy thinking than less politicized types, but in no case were differences statistically significant. We can therefore conclude that the systems analysis variant of an environmentalist explanation does not help to explain our findings.

Our sample size prevents us from investigating other environmental variables. Perhaps some less directly related to the political system play an important role in policy thinking. But this natural tendency to examine one environmental factor after another *ad infinitum* does a genetic explanation something of an injustice. The problem is that while we can examine environmental variables directly, we can usually only infer genetic effects, and so our natural tendency is to slight the latter perspective. In short, our procedures, following the line of least methodological resistance, impinge heavily upon our theoretical perspectives.

A genetic-developmental interpretation presents peculiar inferential difficulties because its major variable is time, which catalogs the maturation of abilities. But men are social creatures; hence, environmental forces constantly interfere with and contaminate genetic maturation.[28] It is therefore impossible to determine the true extent to which such maturation regulates policy thought. Only if we could observe wholly uncontaminated subjects as they age would such an estimation become possible.[29]

Given these problems how can a developmental explanation be applied to our data? The developmental theorist might focus on two aspects of our findings to support an interpretation. First, he would emphasize that age does predict sophistication in policy thinking better than at least one environmental variable—politicization. This fact suggests, but, for reasons we have just reviewed, cannot prove the operation of genetic-maturational factors.

But the developmentalist would still have to explain lack of movement along some dimensions. He might argue, and use recent literature to support his view,[30] that the dimensions which did not change had already developed as fully as possible by eighth-grade. Indeed, perhaps the development of these immobile skills is completed before adolescence. Further, much nonpolitical moral and causal thought also appears to have matured by eighth grade.[31] In short, maybe we should not expect adolescence to produce major changes in all areas of policy-thinking.

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[25] For example, Easton and Dennis contend that, ". . . socialization is . . . one kind of response through which political systems may be able to ward off certain kinds of stress on the structure of political authority . . . ," *Children in the Political System,* p. 388. This observation implies that the political system is a major, and relatively autonomous, socialization agency.

[26] Easton and Dennis, 249–54.

[27] The questions asked were:

How interested would you say you were in politics? Very interested, moderately interested, not very interested. (Scoring—2 points for very interested, 1 for moderately, 0 for not very.)

Have you ever done any campaigning for a political candidate? Yes (3 pts.) No (0 pts.)

Are you a member of any extra-curricular activities having to do with government or current affairs? Yes (2 pts.) No (0 pts.)

How often do you read about politics in the newspapers? Not very often (0 pts.) Moderately often (1 pt.) Very often (2 pts.)

How often do you watch the news on TV? Not very often (0 pts.) Moderately often (1 pt.) Very often (2 pts.)

Finally we asked four questions in order to provide a score based on the respondent's level of knowledge about current political issues. One point was given for each correct answer.

[28] I am indebted to David Sears, who brought this important point home to me with considerable force.

[29] The closest methodological approximation to this ideal is the study of identical twins. For a controversial attempt to generalize from such studies about influences on intelligence, see Arthur Jensen, "How Much Can We Boost IQ and Scholastic Achievement," *Harvard Educational Review,* v. 39 (Winter, 1969), 1–123.

[30] Especially suggestive on this score is the work of Adelson and his colleagues, cited above. See also Judith Gallatin and Joseph Adelson, "Individual Rights and the Public Good," *Comparative Political Studies,* 3, 2 (1970), 226–43.

[31] The work of Piaget is most suggestive on this score. For important cross-national comparisons in causal thinking, see Jerome S. Bruner *et. al., Studies in Cognitive Growth* (New York: Wiley, 1966). A helpful review of some of the tangled terminology used in the

But there are three problems with this position. The first is its tautological character. The interpretation argues that genetic maturation has already occurred when no age-related change is observed, and that genetic maturation also explains all cases where change is observed. In this way aging becomes associated with both positive and negative outcomes; the argument cannot be falsified.[32]

Moreover, the interpretation overstates differences between static and dynamic dimensions. As the index numbers in each table suggest, the static dimensions are not uniformly well-developed; there remains much room for movement towards sophistication. Why then has development either slowed or stopped?

Finally, the developmentalist cannot draw on any theory that postulates selective rather than uniform maturation. The major developmental theories assume that all skills develop in roughly the same way. No influential theorist predicts simultaneous development of some skills and stagnation or even retrogression of others.

We have traveled full circle and find ourselves back in the position we occupied at the beginning of this section. We have convinced ourselves, however, that only a severely modified genetic theory will aid us. Other studies of child and adolescent development have produced a similar conclusion.[33] As Hunt puts it, ". . . Bits of evidence, still limited to be sure, suggest strongly that one is justified in questioning the attribution of the behavioral landmarks of the various stages to maturation."[34]

Our findings demand a developmental theory that allows room for environmental effects and that permits uneven maturation of skills. Let me sketch such a theory:

(1) Modes of policy thought develop naturally at different speeds both within and across individuals.

(2) Political thinking proceeds primarily by genetic maturation when no intense politically related stimuli intersect the development of the mode of thought in question. The best example our findings offer to support this postulate is moral thinking about poverty.

(3) By contrast, political thinking may be a function of politically related stimuli alone when such stimuli are intense, visible, and unequivocal. Under such conditions genetic maturation will not explain political thought. Incidental material drawn from our study, but not related to the nine dimensions of policy thought, led to this conclusion. The author described to each respondent a hypothetical situation in which poor people made demands of government and voiced an intent to destroy property should government prove uncooperative. We were interested in how our respondents expected government to react. Basing our hypothesis on the benevolent leader literature,[35] we predicted that the twelfth-graders would see government as less responsive to the poor than would the eighth-graders. Instead, we uncovered an almost universal belief that government would at best temporize and at worst repress the poor. This general expectation was voiced as commonly by eighth as by twelfth-graders. Typical reactions to the situation were, ". . . I think [the government] would just look at them and say, 'We're going to, huh, you just try to get a riot going, we'll have so many National Guardsmen, so you won't have a chance to do anything." One twelfth grade girl quickly and sadly observed, "They'd bring in the National Guard." When the author inquired into the reasons underlying these negative images, the explanation became clear. Most of our respondents were generalizing from recent events on college campuses and city streets. Some specifically mentioned the Poor People's March on Washington, campus disturbances at the University of Wisconsin, and police actions at the 1968 Democratic convention. In other words,

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area of causal and conceptual thought is Anatol Pikas, *Abstraction and Concept Formation* (Cambridge: Harvard University Press, 1966). Another useful literature review of both moral and causal development is J. G. Wallace, *Concept Growth and the Education of the Child* (The Mere, Upton Park, England: National Foundation for Educational Research in England and Wales, Occasional Publication Series No. 12, 1965), Section III.

[32] On the importance of stating theories in a falsifiable form, see Karl R. Popper, *The Logic of Scientific Discovery* (London: Hutchinson, 1955), Chap. 4.

[33] For one example of the sort of study that has challenged the developmental conception, see W. H. King, "The Development of Scientific Concepts in Children," in Gordon, ed., *Human Development*, pp. 265–76.

[34] J. M. V. Hunt, "The Impact and Limitations of the Giant of Developmental Psychology," in David Elkind and John M. Flavell, eds., *Studies in Cognitive Development: Essays in Honor of Jean Piaget* (New York: Oxford University Press, 1969), pp. 3–67, 52.

[35] See Greenstein, *Children and Politics . . .*, Chap 3; Dennis and Easton *Children in the Political System*, 183. The article which first countered these points of view is Jaros, Hirsch, and Fleron, "The Malevolent Leader . . ." However, this latter research focused on a particular poor subgroup—Appalachian whites—that could be expected to be alienated in some respects. By contrast, we examined middle- and upper middle-class whites from stable suburban and farm backgrounds almost all of whom were motivated scholastically and college-bound.

most had simply been overwhelmed by an image of governmental intransigence in the face of protest. These political stimuli overrode whatever developmental processes might normally be operative. I would expect a similar fate for modes of policy thinking equally vulnerable to political stimulation.

(4) Most political thinking results from a combination of genetic-maturational and politically related environmental factors. Variation in these cases is the product of resonance between environmental stimuli and particular intellectual skills. I would hypothesize, speculatively, that politically related environmental stimuli are probably most likely to impinge on those skills and modes of thought which are salient to the individual at the moment, those which are poorly integrated into the intellectual hierarchy,[36] and those which are developing especially slowly, for these modes of thought are particularly open to the environment. In turn, these skills become the telescopes through which the political world is seen. The result is further variation in policy thinking across people.

Politically related stimuli may have one of three sorts of effects on the modes of thought they intersect. A political stimulus may hasten the speed, integration, or regulation of an intellectual skill. For example, a child's brush with the law may impell him to read up on the legal system, thereby accelerating and solidifying his reading skill. This facilitation effect fits the learning theory offered in Robert White's discussion of ego development.[37]

A second form of resonance is fixation, by which I mean that a powerful politically related stimulus may atrophy the development of a skill, freezing it for the duration of a person's life. For example, an adolescent's vivid and traumatic awareness of a political machine's moral corruption may inhibit the development of his own moral faculties. Fixation is best illustrated by experiments in animal learning.[38]

Finally, a politically-related stimulus may distort the development of a mode of thought. Though development may proceed, it will be diverted into increasingly restricted and tortuous channels. Distortion produced by political stimuli may eventually create considerable psychic stress and subsequent political reaction. For example, an adolescent volunteer's social welfare work may lead him to identify himself wholly, in appearance and in behavior, with clients, thereby putting off others who object to his over-elaboration of role-taking. This form of resonance has been described best, of course, by psychoanalytic theory.[39]

These three sorts of resonance may share a common effect. Each may exaggerate whatever mode of thought it engages, thus stabilizing that mode as the focus through which the individual perceives the political world. Perhaps people seeing the world through the moral mode become political theorists, people seeing politics through the causal mode political analysts, people viewing the political system in the sociocentric mode political activists, and people encountering the political world imaginatively political dreamers.

The ambiguities in our findings foreshadowed the ambiguity of these speculations. The sketch I have presented merely points us toward a conceptualization that captures the interaction of genetic maturation and environmental influences in political thinking. The vagueness of that sketch is a measure of the distance we have yet to go.

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[36] For a conceptual and empirical approach to thought hierarchies, see J. P. Guilford, "The Structure of Intellect," *Psychological Bulletin*, 53, 2 (1956), 267–93.

[37] Robert White, *Ego and Reality in Psychoanalytic Theory; A Proposal Regarding Independent Ego Energies* (New York: International Universities Press, 1963).

[38] For pertinent experimental formulations, see Norman R. F. Maier, *Frustration: The Study of Behavior Without a Goal* (Ann Arbor: Ann Arbor Paperbacks, 1961, first published in 1949); and Justin Aronfreed, *Conduct and Conscience: The Socialization of Internalized Control over Behavior* (New York and London: Academic Press, 1968), Chaps. 3 and 11.

[39] See, especially, Anna Freud, *The Ego and the Mechanisms of Defense* (London: Hogarth Press, 1968).

Political Ethos Revisited

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In *City Politics* we described two contrasting orientations toward politics which we said have decisively influenced attitudes toward both issues and governmental structures in American cities for many decades. The middle-class Anglo-Saxon Protestant ethos, we said, conceived politics as a cooperative search for the implications of an "interest of the whole"; accordingly it stressed the obligation of the individual to participate disinterestedly in public affairs, the desirability of rule by the "best qualified" (meaning "experts" or those otherwise specially equipped by training or character to perform the essentially technical operation of discovering the interest of the whole), and the ideal of "good government" (meaning especially honesty, impartiality, and efficiency). The logically implied (but not always achieved) institutional expressions of this ethos were at-large representation, nonpartisanship, a strong executive (especially the council-manager form), master planning, and strict and impartial enforcement of laws. The other ethos, we said, was characteristic of the immigrant—and therefore of the lower- and working-class—life; it emphasized family needs and personal loyalties and took no account of the larger community; it conceived of politics as competition among individual (that is, family or parochial) interests. The institutional forms that expressed it were ward politics, the boss, and the machine.[1] Both in *City Politics* and later and more elaborately in this *Review* we presented data showing that in referenda, voters in some political districts (wards, precincts, etc.) favored various public expenditures that would confer no benefits on them directly but would cost them something in higher property taxes.[2] We showed that the districts that voted in this "public-regarding" way had higher median incomes and larger proportions of residents of certain ethnic backgrounds (notably Anglo-Saxon, Jewish, and Negro). To be "public-regarding" with respect to local expenditures was, we felt, to display one element of an internally consistent attitudinal structure or ethos that we called, because of what we took to be its origin, middle-class Anglo-Saxon Protestant.

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[1] James Q. Wilson and Edward C. Banfield, *City Politics* (Cambridge: Harvard University Press, 1963), esp. chaps. 3, 16.

Some scholars have drawn from our initial statement of the ethos theory the inference that, if true, there should be discernible in cities a correlation between the proportion of persons with the census attributes associated with having a certain ethos and the presence of institutional forms and urban policies that conform to that ethos. We have argued that such an inference, and hence such a test, is improper because it fails to take into account either the temporal sequence by which electoral attitudes at one period of time are converted into institutional forms that endure in later periods or the numerous political, legal, and institutional variables that intervene between the distribution of attitudes in a population and the choice of local policies or forms of government.

Raymond E. Wolfinger and John Osgood Field ("Political Ethos and the Structure of City Government," *American Political Science Review*, 60 [June, 1966], 306–326) find a relationship between the proportion of persons of foreign stock in 1960 in cities and the existence of "unreformed" political structures (ward elections, mayor-council government, etc.), but that the relationship disappears when region is controlled. Robert L. Lineberry and Edmund P. Fowler ("Reformism and Public Policies in American Cities," *American Political Science Review*, 61 [September, 1967], 701–716) argue that region is a statistically ambiguous control variable, that the influence of an attitude cannot be inferred from the number of persons presumed to hold it, and that 1960 measures of ethnicity are irrelevant to decisions on governmental form made (usually) many decades earlier. They find that in a sample of cities of over 50,000 population in 1960, there are few socioeconomic and only modest ethnic differences between those with "reformed" and "unreformed" institutions.

Robert R. Alford and Harry M. Scoble ("Political and Socioeconomic Characteristic of American Cities," *The Municipal Yearbook, 1965* [Chicago: International City Managers' Association, 1965], pp. 82–97) studying all cities over 25,000 population found a stronger relationship between socioeconomic variables and governmental form than did Lineberry and Fowler, perhaps indicating that the impact of demography on institutional structure is greater in the smaller communities.

Daniel N. Gordon ("Immigrants and Urban Governmental Form in American Cities, 1933–60," *American Journal of Sociology*, 74 [September, 1968], 158–171) discovered a strong relationship between the percentage of foreign born in a city's population and the incidence of "unreformed" institutions for all cities with a 1930 population of 30,000 or more, and noted that the relationship was not attenuated by controls for region, economic base, or population size. The relationship was especially strong in 1930 and 1940, when the foreign-born percentage of urban populations was at its peak.

In this paper we take no position on the issue of the demographic correlates of local political form, though we think it possible in principle, using appropriate historical data and considering the influence of intervening variables, to use the ethos theory as a partial explanation of choices of form.

[2] James Q. Wilson and Edward C. Banfield, "Public-Regardingness as a Value Premise in Voting Behavior," *American Political Science Review*, 58 (December, 1964), 876–887.

In our initial formulation of the ethos theory, some matters were not stated as carefully as they might have been. The "middle-class" Anglo-Saxon Protestants we wrote of in *City Politics* would have been better described as "upper-class" or "upper-middle-class." With reference to the immigrant and working-class ethos, we used the phrase "private-regarding." Though these voters may indeed be private-regarding in the sense of placing their advantage and that of their families ahead of other considerations, we had (and have) no data indicating this. Possibly such voters take into account public needs but define the public differently than do upper-class Anglo-Saxons, thinking of it as their immediate neighborhood or their own racial, ethnic, or occupational group, rather than as the "community as a whole." People who vote against public expenditures do not necessarily do so out of self-interest narrowly conceived; they may simply think that the expenditures in question do not serve "the public interest" as they define it.

Finally, though we argued in the book that historically one ethos was derived from (upper-) middle-class Anglo-Saxon Protestant while the other was derived from immigrant life, it is apparent that it confuses matters unnecessarily to encumber the description of an ethos with conjecture about its historical origins. It is entirely possible that the members of a group might display what we have called the "immigrant" ethos even though none are immigrants; this indeed is what one would expect if the conditions of life that form the native working-class culture are generally similar to those that form the immigrant working-class culture. Furthermore, as immigrants become second- or third-generation and middle-class, they may take on a quite different ethos; we expressly said that a change in class status may lead a group to become assimilated to the pre-existing ethos of that class.[3] Alternatively, ethnic and religious influences may continue to condition the political values of persons of European origin long after those persons have become economically indistinguishable from upper-middle-class Anglo-Saxon Protestants.

To avoid confusion on these matters, we shall henceforth call one syndrome of attitudes about how public affairs should be conducted the *unitary* ethos and the other the *individualist.*[4] The former term refers to what we have previously called the middle-class Anglo-Saxon Protestant ethos or public-regardingness; the latter to what we called the immigrant ethos or (misleadingly) private-regardingness.

The present article defines each ethos in terms of three attitudinal components and then describes the incidence of each as it appeared in a sample of voters in a large city a few years ago. One purpose is to show the proportion of the voters having each attitudinal component and, especially, of those having the right combination of attitudes to constitute an ethos. A second purpose is to identify the attributes (especially ethnicity, religion, income, and years of schooling) of voters having the relevant attitudes or combinations of them. We rely for data on a sample survey rather than, as in our previous article, on voting returns interpreted in the light of census information. This procedure enables us to get the information about attitudes that we need in order to identify those who have one or another ethos and it permits us to say a great deal more than we otherwise could about the ethnic, income and other attributes that are associated with each ethos. By the use of these data we also escape the hazards of ecological correlation—hazards that are considerable even when, as in our previous article, the correlations are quite strong and are confirmed by inspection of the behavior of very small, relatively homogeneous units.

The sample includes 1,059 Boston homeowners who were interviewed in 1966–67.[5] About half the respondents (551) were selected at random on such a basis that every owner-occupied dwelling with no more than three families living in it had an equal chance of being chosen.[6] The other half (508) were selected at random in heavily "ethnic" neighborhoods from among homeowners who met stringent criteria of identity as Negroes, Jews, Italians, Poles, or upper-income white Anglo-Saxon Protestants ("Yankees").[7] Because we would have no need to estimate the value of our variables for the Boston homeowning population, we combined the probability and quota samples for purposes of analysis. To ensure having a sizable number of respondents of the same sex, the male head of the household was interviewed whenever possible.[8] If no male was present, or if for some reason, such as illness, he did not function as the head of the household, we interviewed a female.

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[3] Wilson and Banfield, *City Politics*, p. 42.

[4] These terms were originally developed in the "Note on Conceptual Scheme" in Martin Meyerson and Edward C. Banfield, *Politics, Planning, and the Public Interest* (New York: Free Press, 1955), pp. 303–329.

[5] The survey was supported by grant GS-1836 from the National Science Foundation and by funds from the Rockefeller Foundation and the Joint Center for Urban Studies of MIT and Harvard. None of these organizations, of course, is responsible for our findings and arguments. The authors wish to acknowledge especially the invaluable assistance of Mrs. Susan Michaelson and Mrs. Suzanne Weaver.

[6] For the area sample, one out of every three census tracts in Boston was chosen at random; from within the tracts, every seventh city block was chosen at random. All addresses on these blocks were listed and compared to property tax records kept by the Boston city assessor. All addresses at which more than three families resided or at which the owner did not reside were deleted. From the remaining list was drawn a random sample of 878 owner-occupants of whom 551 were interviewed.

We limited the sample to homeowners because we were particularly interested in the attitudes of those persons who favor public expenditures that would cost them something in property taxes but could not yield them any direct benefit. Because we were going to ask our respondents to "vote" on various public expenditure issues, it was necessary that they be subject to the property tax; a "yes" vote from one who would *not* be taxed might indicate only that he was prodigal with other people's money, not that he was willing to make some sacrifice in order to benefit others or the community. To remind the voter that a "yes" would "cost" him something, we phrased our questions about public expenditures as follows: Do you think we should spend more money on public schools in Boston *even if it costs more in property taxes?*

I. Holists vs. Localists

One attitudinal component of ethos is a disposition to think either in terms of an "interest of the whole" or else in terms of competition among particular—usually local—interests (e.g., family, neighborhood, ethnic group). We shall call those who have the first attitude "holists" and those who have the second "localists." In order to see how many and what kind of respondents fall into these groups, an index was devised from the answers to four questions:[9]

> City officials often have to make decisions in which it is impossible to please everyone. Under these circumstances, which do you think they ought to do—Try to work out a compromise that will give everyone something or do what the experts think is best for the city as a whole even though some people may be satisfied and others hurt by the decision?
>
> In this city, do you think your city councilmen ought to vote mainly to do what is best for your district or ward or to do what is best for the city as a whole, even if it doesn't really help your own district?
>
> As you know, some public officials are elected to office, others are appointed or hired because they are experts or have special training. Which do you feel should have more say in running a city government—officials elected by the people or experts with special training?
>
> Boston has been faced with many problems lately. In looking for men to help solve these problems should we get the best man available anywhere, even though he comes from outside Boston and is not familiar with local problems, or find the best man here in Boston?

One point was assigned for each answer expressing a preference for deciding matters on the basis of what is best for the city as a whole and for relying on the best man available anywhere; a score of zero was assigned each answer indicating a preference for deciding things on the basis of compromise that takes into account neighborhood interests and by means of political judgments made by local men. We shall call "holist" the 45 per cent (475 respondents) who scored three or four and "localist" the 30 per cent (315 respondents) who scored 0 or 1. Those in between (scoring 2) are labelled "nondescripts."

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[7] Respondents in the quota sample were chosen by first selecting those census tracts with the highest proportion of a given ethnic group and then, from these tracts, those blocks with the highest proportion of homeowners. All addresses within those blocks were listed and compared to property ownership records. Respondents were then chosen at random from among homeowners, omiting those who did not fit (where appropriate) particular ethnic criteria (Anglo-Saxon, Italian, Jewish or Polish last names). Yankees were selected only from the three census tracts on and around Beacon Hill with the highest average home value; Italians from four areas (North End, East Boston, West Roxbury, and Hyde Park); Poles from each of three areas in South Boston, the South End, and Dorchester; Negroes from three census tracts in Roxbury; and Jews from those areas with the largest proportion of persons listed as having been born in or having one or both parents born in the USSR. Where ethnic neighborhoods varied by socioeconomic status, three or more areas of differing income levels were used as the sampling frame, with equal numbers of respondents chosen from each.

The criteria for assigning an ethnic identification were as follows: *Negroes:* respondent appeared to be a Negro and described himself as a Negro. *Jews:* respondent gave his religious preference as Jewish. *Italians, Irish, and Poles:* respondent was born in Italy, Ireland, or Poland; or, if American-born, either both his parents or three out of four of his grandparents were born in Italy, Ireland, or Poland, and his religious preference was not Jewish. *Yankees:* respondent described himself as a Protestant, was born in the United States with both of his parents and at least three out of four of his grandparents born in the United States, had an Anglo-Saxon last name, lived in the Beacon Hill area of Boston, and earned more than $10,000 a year.

[8] Of the 1059 respondents, 78 per cent were male. For every ethnic group other than Irish and Negroes, the percentage of female respondents did not exceed 24. For the Irish, females were 32 per cent and for Negroes, 35 per cent.

[9] This index, like the one referring to "good government" versus "benefits" discussed in Section III, is made up of items with high loadings on an orthogonally rotated factor (varimax criterion). Forty attitudinal variables were used in the factor analysis; six factors emerged on rotation, the total communality of which was 15. Some items, thought *a priori* to be related to "holism" versus "localism," were part of the questionnaire but are omitted from the index because of low factor loadings. The loadings of the items used here ranged from +.519 to +.630.

We should expect holists to exhibit preferences predictably different from those of localists on a wide range of matters. Specifically, we should expect them to be more likely to vote "yes" on public expenditures of most (but not all) types, to feel an obligation to participate in public affairs, to favor strict enforcement of laws against gambling and to oppose its legalization, and to think that officials should be honest and impartial no matter what the consequences.

Table 1 summarizes the responses on a variety of such matters. It will be seen that the differences are considerable and that most are in the expected directions. Holists are more likely than localists to vote for most expenditure items. If we divide the items into two "marketbaskets," one consisting of those that are mainly for the support of institutions and the other of those that are mainly for the benefit of concrete groups of persons, the difference in voting is especially marked. Holists are considerably more likely than localists to favor what we shall call (as defined in Section II, below) the "community-serving" marketbasket (*viz.*, increase in salary for mayor and councilmen, support of the zoo, and provision of facilities for juvenile delinquents), whereas localists are more likely than holists to favor most "people-helping" items (*viz.*, bonus for veterans of the Korean War and increase in pensions but not, interestingly, the increase in welfare payments). It will be seen that holists are significantly more likely than localists to describe themselves as "very interested" in state and national politics and to say that they are "very interested" in Boston politics. As we should expect, they take a less tolerant view of gambling than do the localists. Surprisingly, they are less attached to "good government" (as defined in Section III, below) than are the localists.

Table 1. Attitudes of Voters with "Holistic" and "Localistic" Conceptions of the Local Political Process

Per cent Favoring:	Holistic (n=475)	Localistic (n=315)	Non-descript (n=269)
**Schools	82	65	71
Hospitals	78	75	75
**Juvenile facilities	74	65	65
**Zoo	50	38	39
**Mayor-council salaries	29	13	19
Welfare payments	42	38	37
**Veterans' bonus	66	73	72
*Fire and police pensions	37	40	37
**Number of expenditures favored*			
7–8	13	7	8
3–6	75	75	74
0–2	12	18	18
***Type of expenditure favored*			
Community-serving	61	47	44
People-helping	39	53	56
Political participation			
Very interested in:			
**State, national politics	37	27	27
**Boston politics	41	34	32
Not interested in:			
**State, national politics	18	29	23
**Boston politics	19	28	23
Gambling			
**Favor lottery for revenue	63	69	74
*Favor strict enforcement of anti-gambling laws	71	61	69
Favor legalized bookmaking	61	59	59
Good government versus benefits			
Good government-minded	59	65	65
Benefit-minded	41	35	35

* Percentages significant at the .05 level in the chi-square test.
** Percentages significant at the .005 level in the chi-square test.

We should expect holists to be drawn mostly from the upper-middle class and localists to be drawn mostly from the lower-middle and working classes. In income and schooling, then, holists should be clustered at one end of a scale and localists at the other. We should also expect localists to be disproportionately Catholic and Negro. As can be seen from Table 2, these expectations are in general borne out by the data. Holists have more schooling and higher incomes than localists. They are five times more likely than localists to be Yankee and twice as likely to be Jewish. Localists on the other hand are twice as likely as holists to be Italian, Irish, or Negro. Contrary to what we should expect, differences associated with immigrant status are not statistically significant.

II. Community vs. People

A second pair of attitudes that we associate with the unitary ethos and its opposite are "serving the community" and "helping people." We expect one who has the unitary ethos to feel an obligation to contribute to the welfare of unspecifiable individuals or perhaps of a "social organism"; that is what he means by "serving the community." By contrast, we should expect one who has the other ethos to feel disposed to confer benefits ("favors") on more or less specifiable individuals and groups; that is

Table 2. Characteristics of Voters With "Holistic" and "Localistic" Conceptions of the Local Political Process

Characteristics	Holistic (n=475)	Local-istic (n=315)	Non-descript (n=269)
**Income			
Under $5,000	15%	26%	23%
$5,000–7,499	17	33	29
$7,500–9,999	24	17	18
$10,000 and over	40	15	25
NA	4	9	7
**Schooling			
Less than high school	27	50	37
High school	30	36	37
Some college	17	8	13
College graduate	26	6	13
NA	0	2	1
**Ethnicity			
Negro	8	17	15
Jewish	24	11	17
Italian	7	18	12
Polish	8	13	8
Yankee	10	2	3
Irish	8	15	11
Other	35	25	34
**Religion			
Protestant	27	21	27
Catholic	44	67	49
Jewish	26	11	18
Other, NA	3	1	5
Immigrant generation			
Immigrant	19	27	22
First generation	45	39	45
Second generation or more	32	29	29
NA	4	5	4
Religiosity			
**Strong religious feeling	27	37	32
**Very religious upbringing	47	61	53
**Attend church weekly or oftener	47	67	45

** Percentages significant at the .005 level in the chi-square test.

what he means by "helping people." To see what proportion of our respondents have these attitudes, we sorted them according to whether they favored the "community-serving" or "people-helping" marketbaskets of expenditures.

Expenditures that we consider primarily "people-helping" are the veterans' bonus, pensions for policemen and firemen, and higher welfare payments. Those that we consider primarily "community-serving" are the zoo, facilities for juvenile delinquents, and salary increases for the mayor and city councilmen.[10] We regard the last item as community-serving because, although the salaries go to particular people, its supporters most frequently justify it on the grounds that higher salaries will "attract better people" to the job or make graft-taking less likely. Community-servers are those respondents who, if they voted "yes" on five or more items, included the three community-serving ones among them and if they voted "no" on three or more included the three people-helping items among them. People-helpers are those who, if they voted "yes" on six or more items, included the people-helping ones among them and if they voted "yes" on fewer than six did not include any community-serving items.

Twenty-five per cent of the sample (268 respondents) turned out to be community-serving and 23 per cent (238 respondents) people-helping.

If the voting on these items does in fact reflect the differences in attitude that we are trying to get at, we should find some evidence of it in the reasons given by the respondents for voting as they did. With respect to three of the eight items, there are no differences in the reasons given. With respect to the other five, there are important differences, as can be seen from Table 3. Generalizing these findings, it is fair to say that community-servers are more likely than people-helpers to think in terms of "needs of society" (or "costs to society") and of "responsibilities of society"; they are also more likely to be aware of what is required for the maintenance and management of institutions, and to think in terms of general principles of obligation—for example, the obligation to be equitable and impartial. People-helpers, on the other hand, tend to be more sensitive to the needs and wishes of concrete groups and individuals and families rather than to abstract entities like society, and to favor kindness over fairness.

It should be stressed that we defined the orientation toward public expenditures in such a way that each voter's marketbasket of preferred items might vary slightly. To be community-serving or people-helping one need not choose exactly the same marketbasket as everyone else with the same attitude. Voters with the same general outlook on public affairs may differ somewhat in their judgments about particular expenditure items; if our definitions were narrower they would be of particular rather than

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[10] Some readers may feel that spending money on facilities to handle juvenile delinquents is "people-helping" as much as "community-serving." In a sense that is true and, as Table 3 shows, the motives for supporting it contain elements of both. The distinguishing feature for classificatory purposes is that (unlike the bonus and welfare payments) it does not give the beneficiaries *what they themselves want*—e.g., money. Two expenditure items—the schools and the city hospital—are unclassifiable in that they serve *both* the community (educating citizens and checking disease) and individual recipients (acquiring earning power and receiving medical care); these two items were omitted from the two marketbaskets and, as we shall see, voters of both types seemed to support the schools and hospital in about equal proportions.

Table 3. Reasons Most Commonly Given by Community-Serving and People-Helping Voters in Explaining "Yes" and "No" Votes on Expenditure Issues

Expenditure Issues	Reasons	Community-Serving Voters	People-Helping Voters
Schools			
"Yes" reasons	"community needs better schools"	82%	71%
	"it will help the children"	18	29
		(n=192)	(n=157)
"No" reasons	"taxes are too high"	44%	45%
	"schools are poorly run"	35	31
		(n=54)	(n=65)
Juvenile facilities			
"Yes" reasons	"help and protect society"	53%	39%
	"help children"	29	50
		(n=169)	(n=64)
"No" reasons	"taxes too high"	29%	19%
	"it's the parents' responsibility"	27	42
		(n=48)	(n=122)
Veterans' bonus			
"Yes" reasons	"they did something for us"	56%	71%
	"it's only fair, since others have gotten it"	33	22
		(n=77)	(n=209)
"No" reasons	"it's their duty; shouldn't be paid for it"	32%	27%
	"they already are paid enough"	29	36
		(n=105)	(n=11)
Welfare payments			
"Yes" reasons	"recipients need it"	80%	93%
	"society has an obligation"	20	7
		(n=40)	(n=94)
"No" reasons	"poorly administered"	28%	15%
	"recipients are immoral"	26	41
		(n=182)	(n=135)
Hospital			
"Yes" reasons	"to help the poor"	42%	50%
	"facilities are inadequate"	40	29
		(n=172)	(n=166)
"No" reasons	"poorly administered"	40%	43%
	"taxes too high"	29	30
		(n=55)	(n=40)

of general attitudes. The consequences of using our flexible, probabilistic definition can be seen at the top of Table 4. By definition, of course, the community-servers and people-helpers vote disproportionately for the items that are primarily community-serving or people-helping. But some voters in each group cross over to pick an item from the other marketbasket. The ones most often picked are the juvenile facilities (by about a third of the people-helpers) and the veterans' bonus (by about a third of the community-servers). The schools and the hospital get about equal support from both groups.

As Table 4 shows there are significant differences of the kinds that we should expect in the responses of the two groups to many questions. The community-servers are more likely than the people-helpers to say that they are "very interested" in state and national and in Boston

Table 4. Attitudes of Voters Preferring "Community-Serving" and "People-Helping" Public Expenditures

Per cent Favoring:	Community Serving (n=268)	People-Helping (n=238)
Schools	76%	68%
Hospital	71	77
Juvenile facilities	75	32
Zoo	58	16
Mayor-council salaries	53	7
Welfare payments	22	58
Veterans' bonus	30	92
Fire and police pensions	22	58
** *Number of expenditures favored*		
7–8	25	16
3–6	41	59
0–2	34	26
Political participation		
Very interested in:		
** State, national politics	38	28
Boston politics	37	38
Not interested in:		
** State, national politics	17	29
Boston politics	21	25
Gambling		
** Favor lottery for revenue	55	74
* Favor strict enforcement of anti-gambling laws	72	65
Favor legalized bookmaking	56	62
** *Structure of political process*		
Holistic	56	40
Localistic	23	30
Other	21	30
Good government versus benefits		
Good government-minded	66	62
Benefit-minded	34	38

* Percentages significant at the .05 level in the chi-square test.

** Percentages significant at the .005 level in the chi-square test.

politics. As we should expect, they are more likely to think that decisions should be made on a holistic rather than localistic basis and to be (slightly) less likely to trade "good government" for some increase in benefits that "help people."

The attributes of the community-servers and the people-helpers are shown in Table 5. As we should expect, the former tend to have higher incomes and more schooling. Half of them, but only one-seventh of the people-helpers, have one or more years of college; almost two-thirds of them, but only less than half of the people-helpers, earn more than $10,000 a year. Yankees and Jews are, as we should expect, over-represented among the community-servers and under-represented among the people-helpers. Somewhat to our surprise, the Poles are about evenly divided. Immigrant status is not significantly associated with the choice between "community" and "people." As we should expect, the religious involvement of people-helpers is greater than that of community-servers.

III. Good Government vs. Benefits

The third attitudinal component of each ethos has to do with the willingness of the individual to accept some sacrifice of the "good government" virtues (honesty, impartiality, and efficiency) for the sake of greater substantive results or achievement. We shall refer to those having these attitudes as "good government-minded" or "benefit-minded." (We do not intend to imply value judgments; the former could be called rule-oriented and the latter result-oriented.) In order to see how many respondents have one or the other of these attitudes we sorted them out according to their answers to the following questions.[11]

> A mayor who gets things done but takes a little graft is better than a mayor who doesn't get much done but doesn't take any graft. [Agree, disagree.]
>
> If a politician helps people who need it, it doesn't matter that some of the rules are broken. [Agree, disagree.]
>
> If you had to choose, which would you prefer—a city run by politicians who maintained a good school system but made money for themselves by doing it or a city run by honest officials who maintained a school system that wasn't quite so good?

Sixty-two per cent of the sample (658 respondents) answered two or three questions in the "good government" manner; 38 per cent (401 respondents) gave "benefit-minded" answers to two or three questions.

As can be seen from Table 6, good government-minded voters do not differ very much from benefit-minded ones on the number or kinds of expenditure items that they support. Nor do the two groups differ significantly in the interest that they profess to take in Boston or in state and national politics. On certain other questions, however, they clearly differ. The good government-minded voters are less likely to favor creation of a state lottery or making off-track betting legal and they are more likely to want existing laws against gambling strictly enforced. Apparently this pair of attitudes has reference solely to a certain kind of issue (viz., standards of propriety for both officials and citizens), one which has little or nothing to do, as the voter sees it, with whether experts are to be preferred to politicians or expenditures should serve the community or help people. As we shall presently explain, little association exists between this attitudinal component and the others of each ethos. Of that little, some is the reverse of what we should expect.

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[11] The factor loadings of the three items used to construct this index ranged from +.664 to +.771.

Table 5. Characteristics of Voters Preferring "Community-Serving" and "People-Helping" Public Expenditures

Characteristics	Community-Serving (n=268)	People-Helping (n=238)
**Income		
Under $5,000	15%	27%
$5,000–7,499	19	23
$7,500–9,999	19	21
$10,000 and over	43	21
NA	6	8
**Schooling		
Less than high school	22	47
High school	27	38
Some college	16	8
College graduate	34	7
NA	2	0
**Ethnicity		
Negro	9	12
Jewish	19	16
Italian	9	13
Polish	10	11
Yankee	14	3
Irish	9	12
Other	31	33
**Religion		
Protestant	34	22
Catholic	43	58
Jewish	21	16
Other, NA	3	3
Immigrant generation		
Immigrant	18	25
First generation	42	42
Second generation or more	35	29
NA	5	3
Religiosity		
Strong religious feeling	29	35
*Very religious upbringing	48	58
Attend church weekly or oftener	51	58

* Percentages significant at the .05 level in the chi-square test.

** Percentages significant at the .005 level in the chi-square test.

Table 6. Attitudes of Voters Who Are "Good Government-Minded" or "Benefit-Minded"

Per cent Favoring:	Good Government-Minded (n=658)	Benefit-Minded (n=401)
*Schools	71%	79%
Hospital	75	79
Juvenile facilities	70	68
Zoo	43	45
Mayor-council salaries	21	22
Welfare payments	40	39
Veterans' bonus	67	73
Fire and police pensions	38	38
Number of expenditures favored		
7–8	10	10
3–6	74	76
0–2	16	14
Type of expenditure favored		
Community-serving	54	51
People-helping	46	50
Political participation		
Very interested in:		
State, national politics	33	29
Boston politics	39	33
Not interested in:		
State, national politics	22	23
Boston politics	22	25
Gambling		
**Favor lottery for revenue	63	75
**Favor strict enforcement of anti-gambling laws	73	57
**Favor legalized bookmaking	56	67
Structure of political process		
Holistic	43	49
Localistic	31	28
Other	26	24

* Percentages significant at the .05 level in the chi-square test.

** Percentages significant at the .005 level in the chi-square test.

Table 7. Characteristics of Voters Who Are "Good Government-Minded" or "Benefit-Minded"

Characteristics	Good Government-Minded (n=658)	Benefit-Minded (n=401)
Income		
Under $5,000	21%	19%
$5,000–7,499	25	25
$7,500–9,999	19	22
$10,000 and over	29	28
NA	6	6
Schooling		
Less than high school	34	39
High school	33	33
Some college	13	13
College graduate	18	15
NA	1	0
**Ethnicity		
Negro	12	14
Jewish	15	24
Italian	10	14
Polish	11	7
Yankee	7	4
Irish	11	10
Other	33	29
**Religion		
Protestant	27	22
Catholic	54	49
Jewish	16	25
Other, NA	2	4
Immigrant generation		
Immigrant	22	23
First generation	42	45
Second generation or more	31	28
NA	5	5
Religiosity		
**Strong religious feelings	34	27
Very religious upbringing	54	50
**Attend church weekly or oftener	59	48

** Percentages significant at the .005 level in the chi-square test.

The attributes of the good government-minded and benefit-minded respondents are shown in Table 7. There is no significant relationship between either schooling or income and having one attitude or the other. Ethnic differences, however, are quite pronounced. The benefit-minded are disproportionately Italian, while the good government-minded are disproportionately Yankee. Indeed, of all the Yankees in the sample, 76 per cent are in the good government category. The Jews, somewhat surprisingly, are almost equally divided between the good government-minded and the benefit-minded and they make up nearly a quarter of the latter group (but less than a sixth of the former). With respect to the holistic and localistic attitudes, it will be recalled, the Jewish distribution resembles that of the Yankees. A second surprise is that the good government-minded are disproportionately Polish and Irish. With respect to the other pairs of attitudes, they are (like the Negroes and Italians) disproportionately people-helpers and localists.

IV. Attitude in Relation to Ethos

To have an ethos as we defined it originally in *City Politics*,[12] a voter must have the "right" three attitudes in combination. The "right" combination derives from our view that there is a quasi-logical relation ("pattern harmony," perhaps) among the attitudes such that a voter who sees politics in terms of an interest of the whole (i.e., a holist) is likely to emphasize both community-serving expenditures and good-government virtues, whereas one who sees politics as competition among local interests (i.e., a localist) is likely to emphasize both people-helping expenditures and results rather than rules. In other words, we originally hypothesized that having one attitude implies, perhaps weakly, having the two that "go with" it and, more strongly, *not* having either of the two implied by its opposite.

We hasten to add that we do not expect this quasi-logical or predicted relation either to be displayed on all occasions or, when it is displayed, always to be decisive in the behavior of the voter. For one thing, the relation is, as we have said, a weak one. For another, it may be that some persons will perceive a logic different from the one that we perceive, or perhaps no logic at all. What is perhaps more important, attitudes are merely *dispositions* to act in certain ways; even when a disposition is strong, other factors may offset it. Real political choices can seldom be made solely on the basis of abstract principles (ethos, ideology, etc.). In national surveys, policy attitudes are not usually bound together by underlying abstractions and only a small fraction (though perhaps an important one) of the electorate has an "ideological" conception of political matters.[13] Finally, survey techniques cannot ensure that a respondent will evaluate the hypothetical situation by no other standards than those the investigator had in mind. For example, a unitarist might, even though strongly disposed to favor nonpartisan elections as a general thing, favor partisan ones in the interview situation because in the concrete circumstances which he takes the question to presuppose (i.e., the realities of Boston politics as he conceives them) he believes partisan elections are more likely to serve ends (e.g., the election of a strong mayor) instrumental to his unitarist values.

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[12] Wilson and Banfield, *City Politics*, p. 235.

[13] See, for example, David O. Sears, "Political Behavior," in Gardner Lindzey and Elliot Aronson, eds., *The Handbook of Social Psychology* (2nd ed.; Reading, Mass.: Addison-Wesley Publishing Co., 1969), Vol. V, p. 330; Angus Campbell, Philip E. Converse, Warren E.

For all these considerations we think it would be unreasonable to expect those voters who have some of the attitudes in question to *display* them *all* in the "right" combination in *every* choice situation. Even given optimal measures of attitudes, we would look for no more than (in William Sumner's words) a "strain toward consistency." That is, we would expect some positive correlation among the "right" attitudes.

The data do show a "strain toward consistency," but a weaker one than we expected. Ignoring for a moment that the attitudinal indices are not interval scales, the simple correlation coefficient between the respondents' position on the holism index and whether or not they are community servers is +.135 (significant at the .01 level); the correlation between holism and stressing good government is +.058 (not significant at the .05 level). Stated another way, of the 268 voters who are community servers, 151 (56 per cent) are also holistic and of those, 97 (64 per cent) are good government rather than benefit-minded.

Taking the sample of 1,059 respondents as a whole, 12 per cent display the "right" three attitudes in combination; if we define ethos in terms of only two attitudes (dropping the good government versus benefit one), 21 per cent display the "right" combination. It should be borne in mind, however, that both the degree of overlap among attitudes and the proportion of the sample having the "right" combination is affected by the definition of the attitudinal indices and the placement of the cutting points on these scales. The holism index is a trichotomy, the middle group of which is ignored; were the index a dichotomy, the overlap between it and other indices would be greater. And the community-serving versus people-helping index is defined so as to exclude a number of logically-possible combinations of expenditures (chosen, as it turns out, by about half the sample); a less stringent definition of these attitudes would again increase both the overlap and the proportion of respondents with that attitude.

Allowing for all these methodological problems, however, it remains true that a significant number of voters displays one attitude without displaying either of the other two. The good government attitude, which we supposed to be logically related to the holism and community-serving attitudes, is unrelated to them for most voters. Fewer than half the good government-minded voters are holists and barely half of them are community servers; of the 71 voters who are both people helpers and localists, 24 (34 per cent) have the predicted attitude of benefit-mindedness.

V. Ethos and Attributes

The ethos theory asserts not only that some voters will display the different, and opposed, sets of attitudes that we have described but also that these differences of ethos will be associated with differences in certain attributes, especially ethnicity, religion, schooling, and income. We should expect Yankees and Jews to be disproportionately unitarists and persons of immigrant-Catholic background (members of what Mayor Curley called "the newer races") to be disproportionately individualistic. Similarly, we should expect the relatively well-schooled and well-off to tend to be disproportionately unitarist and the less well-schooled and well-off to be disproportionately individualist. The longer the process of cultural assimilation goes on, the weaker we should expect these relations to become, but our conjecture is that although strongest in first generation immigrants, they are by no means feeble in other voters.

Allowing for some anomalies, the data are consistent with our expectations. Table 8 shows that the unitarists include a disproportionate number of Yankees and Jews and also of the well-schooled and well-off; it shows also that the individualists include a disproportionate number of Irish, Italian, and Polish Catholics as well as of the less well-schooled and the relatively low income (it should be remembered that all of our respondents are homeowners: that is, none are really poor).

In Table 9 we show these relationships with unitarist and individualist defined by two rather than three attitudes (the good government versus benefit one being dropped). This gives us a considerably larger *n*, but the relationships remain essentially the same.

Miller, and Donald E. Stokes, *The American Voter* (New York: John Wiley & Sons, 1960), pp. 216–265; Philip E. Converse, "The Nature of Belief Systems in Mass Publics," in David E. Apter, ed., *Ideology and Discontent* (New York: Free Press, 1964), pp. 218, 225, 255; and V. O. Key, Jr., *Public Opinion and American Democracy* (New York: Alfred A. Knopf, Inc., 1961), pp. 154–172.

Table 8. Characteristics of Voters Who Are "Holistic/Community-Serving/Good Government-Minded" or "Localistic/People-Helping/Benefit-Minded"

Characteristics	Holistic, Community-Serving, Good Government-Minded (n=97)	Localistic, People-Helping, Benefit-Minded (n=26)
**Income		
Under $5,000	12%	27%
$5,000–7,499	8	15
$7,500–9,999	17	19
$10,000 and over	59	31
NA	4	8
**Schooling		
Less than high school	12	27
High school	22	54
Some college	19	8
College graduate	46	8
NA	1	4
**Ethnicity		
Negro	4	8
Jewish	14	35
Italian	3	23
Polish	9	0
Yankee	24	0
Irish	8	15
Other	37	19
**Religion		
Protestant	42	15
Catholic	38	46
Jewish	18	35
Other, NA	2	4
*Immigrant generation		
Immigrant	11	27
First generation	35	54
Second generation or more	47	15
NA	6	4
Religiosity		
Strong religious feelings	24	23
Very religious upbringing	44	54
*Attend church weekly or oftener	49	54

* Percentages significant at the .05 level in the chi-square test.

** Percentages significant at the .005 level in the chi-square test.

One major difference between Tables 8 and 9 is that the number of Jewish unitarists is reduced when ethos is defined by three attitudes instead of two. This occurs because so many Jewish respondents are benefit-minded. This finding is anomalous in the light of our other findings. Income makes no difference and schooling makes very little difference in the probability that a voter who is both holistic and community-serving will also be good govern-

Table 9. Characteristics of Voters Who Are "Holistic/Community-Serving" or "Localistic/People-Helping"

Characteristics	Holistic and Community-Serving (n=151)	Localistic and People-Helping (n=71)
**Income		
Under $5,000	11%	38%
$5,000–7,499	11	21
$7,500–9,999	17	17
$10,000 and over	58	14
NA	3	10
**Schooling		
Less than high school	15	66
High school	22	30
Some college	19	1
College graduate	44	3
NA	1	0
**Ethnicity		
Negro	4	18
Jewish	23	11
Italian	4	17
Polish	9	17
Yankee	20	1
Irish	6	14
Other	34	21
**Religion		
Protestant	38	21
Catholic	34	68
Jewish	25	11
Other, NA	2	0
*Immigrant generation		
Immigrant	12	30
First generation	41	35
Second generation or more	42	31
NA	5	4
Religiosity		
*Strong religious feelings	24	38
*Very religious upbringing	45	68
*Attend church weekly or oftener	42	68

* Percentages significant at the .05 level in the chi-square test.

** Percentages significant at the .005 level in the chi-square test.

ment-minded, but ethnic difference—specifically being Jewish or not—makes a striking difference. Benefit-minded respondents are almost three times as likely as good government-minded ones to be Jewish (Table 10). Stated another way, of the 35 Jewish respondents who are both holistic and community-serving, only 40 per cent are good government-minded whereas of the 30 Yankees who are both holistic and community-serving 77 per cent are good government-minded.

Table 10. Characteristics of Voters Who Are "Good Government-Minded" or "Benefit-Minded" Among Those Who Are Both Holistic and Community-Serving

Characteristics	Holistic and Community-Serving Voters	
	Good Government-Minded (n=97)	Benefit-Minded (n=54)
Income		
Under $5,000	12%	7%
$5,000–7,499	8	17
$7,500–9,999	17	19
$10,000 and over	59	57
NA	4	0
Schooling		
Less than high school	12	20
High school	22	22
Some college	19	19
College graduate	46	39
NA	1	0
*Ethnicity		
Negro	4	4
Jewish	14	39
Italian	3	6
Polish	9	9
Yankee	24	13
Irish	8	2
Other	37	28

* Percentages significant at the .05 level in the chi-square test.

The other anomaly we mentioned above, i.e., that only 34 per cent of those who are localistic *and* people-helping are benefit- rather than good government-minded. This (to us) unexpected emphasis on good government is strong among both Polish and Irish respondents. These are groups that tend to report having had a religious upbringing and that tend also to have relatively little schooling and income. We conjecture that this background may dispose many of them to respond in interview situations on the basis of a highly conventionalized, or "textbook," code of civic morality which tells them that "rules are rules" and makes no provision (as would utilitarianism, for example) for dealing with difficult cases. If we are right about this, these voters' endorsement of good government probably has little value as an indicator of the choices that would be made in "real life" situations. Our interpretation leaves us with an awkward question, however: Why do other voters having these same attributes (low income, little schooling, religious upbringing)—notably the Italians and the Negroes—not display an equal enthusiasm for good government?

VI. Sources of Ethos

If ethnicity, religion, schooling, and income are all associated with ethos, what is their relative explanatory value? Could we not, perhaps, find a single variable—say income—that is so closely associated with the various attitudes and combinations of attitudes that other variables could be ignored? In other words, may not ethos be for all practical purposes a proxy for, say, high or low income?

We intend to go into these questions in another publication. Some things, however, may be said here.

In Table 11 we show the effect of income on ethos when schooling is controlled for among respondents with no more than a high school diploma, and in Table 12 we show the effect of schooling when income is controlled for among respondents earning less than $7,500 a year. In both tables, the proportion of persons with a unitarist ethos (defined here by only two attitudes) increases with increases in the test variable. Although the relationships are statistically significant, it is apparent that substantial differences exist only at the extremes. Among persons earning less than $5000 and with less than a high school education, 14 per cent are individualists and only 5 per cent unitarists while among those with that amount of schooling but earning more than $10,000 unitarists account for 16 per cent and individualists for only 8 per cent (Table 11). In the middle income range ($5,000–$10,000) income is not significantly associated with ethos when schooling is held constant.

Stated another way, disproportionate numbers of individualists exist only among those who rank lowest, and disproportionate numbers of unitarists only among those who rank highest in income and amount of schooling. Two circumstances help to explain these findings. First, one component attitude of ethos (holism versus localism) is affected only by very large differences in income or schooling. There are

Table 11. Effect of Income on Ethos, Controlling for Schooling

Ethos and Attitudes	Respondents With High School Education Or Less Who Earn: $0–$4999 (n=193)	$5000–$7499 (n=224)	$7500–$9999 (n=155)	$10,000 plus (n=121)
*Ethos defined by two attitudes				
Unitary	5%	6%	8%	16%
Individualist	14	6	7	8
Ethos defined by three attitudes				
Unitary	3	3	5	10
Individualist	3	2	3	5
Attitudes				
**Holist	31	30	47	53
Localist	42	42	30	23
Community-serving	15	18	20	21
People-helping	31	22	28	29
Good government	64	60	58	61
Benefits	36	40	42	39

* Percentages significant at the .05 level in the chi-square test.
** Percentages significant at the .005 level in the chi-square test.

no significant differences in the percentage of holists in the first two columns of either table; holism is frequent only among those with some college experience (income being controlled) and only among those earning more than $7,500 (schooling being controlled). Second, another component attitude of ethos (community-serving versus people-helping) is associated with differences in schooling but not with differences in income. Holding schooling constant, a voter is no more likely to be community-serving as his income increases; however, holding income constant, the more schooling he has the more likely he is to be *both* community-serving and holist. Incidentally, neither income nor schooling affects the likelihood of his having one rather than the other of the "good government versus benefit" attitudes.

Table 12. Effect of Schooling on Ethos, Controlling for Income

Ethos and Attitudes	Respondents Earning Under $7500 Per Year With: Less Than High School (n=253)	High School Only (n=164)	Some College or More (n=57)
*Ethos defined by two attitudes			
Unitary	5%	7%	14%
Individualist	14	4	2
Ethos defined by three attitudes			
*Unitary	2	4	11
Individualist	2	2	2
Attitudes			
*Holist	29	32	47
Localist	45	37	23
*Community-serving	15	19	30
People-helping	29	22	16
Good government	63	62	70
Benefits	38	38	30

* Percentages significant at the .05 level in the chi-square test.

That unitarists are represented disproportionately among voters with the most schooling and income and individualists disproportionately among those with the least is consistent with and supportive of the findings we reported in 1964 on the basis of our study of voting behavior on public expenditures. In that article we said that the behavior of the upper-income voters, particularly the Anglo-Saxon Protestants and the Jews among them, could not be rationalized on the assumption that they were trying to maximize self-interest narrowly conceived; for this reason we called these voters public-regarding.

Our present data include two indicators of self- versus public-regardingness that deserve mention in this connection. One has to do with public school expenditures. Some respondents have children in the public schools and others do not. We find that those with children in the public schools are only slightly more likely than the others to favor increased expenditures for schools (81 as compared to 74 per cent). We also find that support for increased school expenditures (which is quite high generally) increases with income *whether or not the respondent has children in the schools.* Thus those *without* children in the schools and earning more than $10,000 voted for increased expenditures by the same large majority (77 per cent) as those *with* children in the schools and earning less than $5,000. The other indicator of self- versus public-regardingless concerns the veterans' bonus. We find that respondents who are veterans are a little more likely to vote for the bonus than those who are not—75 per cent as against 68. As we should expect from our previous study, income and schooling have much to do with whether or not a veteran votes against his self-interest narrowly defined. Among veterans, 80 per cent of those earning under $5,000 favor the bonus whereas only 66 per cent of those earning more than $10,000 favor it. Schooling makes even more difference: among veterans without a high school diploma, 79 per cent favor the bonus whereas among those having a college degree only 48 per cent do.

We have tried to isolate the effect of ethnicity by controlling simultaneously for income and years of schooling. We can do this with only two "social classes," one composed of voters who have not gone beyond high school and are middle income, the other of ones who are college graduates and upper income. (Unfortunately our sample is such that the first class includes no Yankees and too few Poles to permit tabulation; and the second includes hardly any Negroes and so few Irish, Italians, and Poles that we have lumped them together as "European Catholics.") Table 13, then, shows the effect of ethnicity for selected ethnic groups when "class" is controlled for.

In the lower of these two classes, Jewish voters are distinctive. They are more than twice as likely to be unitarists as to be individualists, whereas the Negroes, Italians, and Irish in this class are three to four times more likely to be individualists than unitarists. One of the main reasons for this showing is that the Jews are less likely than the others to be localistic (only 31 per cent as compared to 46 to 55 per cent of the others). In this social class, ethnicity is not associated with either the community-serving or the people-helping attitudes. But it is associated —unexpectedly—with the good government attitude. As we remarked above, Polish and Irish voters are significantly more likely than others to have this attitude.

In the upper class, ethnic effects are striking. When ethos is defined by only two attitudes, Yankees are significantly more likely than Jews or European Catholics to be unitarists. They are also more likely to be unitarists when ethos is defined by all three attitudes, but under this definition, owing to the small numbers involved, the relationship just fails of statistical significance at the .05 level. Yankees and Jews

Table 13. Ethos and Attitudes by Ethnicity, Controlling for Income and Schooling

Ethos and attitudes	Income Under $7500; High School Graduate or Less				Income Over $10,000; College Graduate or More		
	Negro (n=77)	Jew (n=51)	Italian (n=75)	Irish (n=52)	Jew (n=28)	Yankee (n=44)	Catholic (n=29)
Ethos defined by two attitudes							
Unitary	* 3%	14%	3%	4%	*36%	57%	28%
Individualist	12	6	12	12	0	0	0
Ethos defined by three attitudes							
Unitary	1	4	1	4	11	41	21
Individualist	1	10	3	0	0	0	0
Attitudes							
Holist	*27	37	17	29	*82	82	52
Localist	47	31	55	46	4	5	24
Community-serving	16	24	23	17	*36	71	52
People-helping	25	26	24	25	7	5	10
Good government	*53	53	53	71	46	71	69
Benefits	47	47	47	29	54	29	31

* Percentages in these rows are significant at the .05 level on the chi-square test.

of the upper class are about equally likely to be holistic (European Catholics are much less likely to be); Yankees and European Catholics are about equally likely to be good government-minded (Jews are much less likely to be); only the Yankees are very likely to be community-serving.

The Yankees display the unitarist ethos in a uniquely strong and consistent way. Although they are only 6 per cent of the sample, they account for 20 per cent of those who are both holistic and community-serving and for 24 per cent of those who are holistic, community-serving and good government-minded. A trifle more than half (51 per cent) of the Yankees are unitarist as defined by the first two attitudes and more than a third (39 per cent) are unitarist as defined by all three. They constitute the stable core of what may be viewed as a set of coalitions among upper class voters. When the question is between an "interest of the whole" and competing individual interests, the Yankees are joined by most of the Jews, with the Catholics about equally divided. When, however, the question is whether to maintain good government principles even at a sacrifice of larger benefits or results, the Yankees are joined mainly by Catholics (especially Irish and Polish ones) with a majority of the Jews taking the other side. And when the question is whether to emphasize expenditures that "serve the community" or ones that benefit specifiable individuals, the Yankees draw what support they can—it is not much—from a variety of sources.

VII. Summary and Significance

There was found in a sample of predominately male Boston homeowners two distinctive political orientations that expressed attitudes about the appropriate scope, goals, and principles of local governmental action. One ethos, the "unitarist" is composed of holistic and community-serving conceptions of the public interest; the other, the "individualist," consists of localistic and people-helping conceptions of the public good. About one fifth of the respondents displayed one or the other ethos and had in addition the predicted personal attributes. Unitarists were much more likely to be Yankee or Jewish and to have higher incomes and more schooling than were either the individualists or those respondents with no distinctive ethos.

Contrary to prediction, having good government attitudes was not found for the sample as a whole to be strongly associated with the unitarist ethos, and being benefit-minded was not associated with the individualist ethos. These anomalies resulted from the fact that, unexpectedly, Jewish respondents were more likely to be benefit-minded than good government-minded while Irish and Polish Catholic respondents with relatively low incomes and little schooling were more likely to be good government-minded than benefit-minded.

Income, schooling, and ethnicity appear to have an independent association with each ethos and with at least two of the three component attitudes. Upper-income Yankee respondents were distinctive in having, to an exceptional degree, all three attitudes comprising the unitarist ethos.

We think it likely that in times past each ethos was both more widespread and less often held in incomplete or attenuated form. When American city government was being organized into the forms we have today (in many cities this was in the late nineteenth and early twentieth centuries), differences in income and schooling were greater than they are now and conflict between old-stock Americans and the "newer races" was much more intense. The result may have been that persons with one or another well-defined ethos were more numerous among political activists and also that they were more keenly aware of their own and their opponents' distinctive attitudes. However this may be, it is clear that the processes of assimilation have not by any means eradicated the differences that were so conspicuous a few decades ago.

The claims that we made in *City Politics* about the historical importance of ethos did not, however, depend upon either ethos having been widespread in the electorate. In many cities two small cliques, one of Yankee businessmen and professionals and the other of working- and lower-middle class politicians of immigrant origins, have, first one and then the other, exercised power that was largely independent of the opinion of the ordinary citizen. If, as we surmise, members of these cliques were (and *are*, for in many cities the situation is not very different from what it was) more likely than the ordinary citizen to display one or another ethos in well developed form, the explanatory value of the ethos concept may well be greater than our present data on the incidence among Boston homeowners of one or another attitude or combination of attitudes might suggest. Indeed, as we intend to show in another publication, the unitarists (and possibly also the individualists) in our sample, although a tiny part of the whole Boston electorate, do have an importance in the city's affairs far out of proportion to their numbers.

Nondecisions and the Study of Local Politics*

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i do not think a prudent one
will ever aim too high
a cockroach seldom whips a dog
and seldom should he try

and should a locust take a vow
to eat a pyramid
he likely would wear out his teeth
before he ever did

i do not think the prudent one
hastes to initiate
a sequence of events which he
lacks power to terminate
archy and mehitabel

We Germans who know Germany know better what is good for the Alsatians than those unfortunate people themselves, who, warped by their French life, have remained without honest knowledge of the new Germany. We want to give them back their own self against their will.
Heinrich von Treitschke

With the appearance in 1962 of "Two Faces of Power,"[1] Peter Bachrach and Morton S. Baratz initiated a new round in the continuing controversy pervading the field of "community power."[2] Their article was based on Robert A. Dahl's *Who Governs*, a study of (among other things) important decisions made by the municipal government in New Haven.[3] Dahl had defined power this way: "A has power over B to the extent that he can get B to do something that B would not otherwise do."[4] He reported that in New Haven "only a small number of persons have much *direct* influence, in the sense that they successfully initiate or veto proposals for policies,"[5] but that political power was both decentralized and specialized. More important, the greatest measure of influence was in the hands of the popularly elected mayor, not a cabal of businessmen. Bachrach and Baratz argued that Dahl had not adequately tested the hypothesis that a ruling elite really controlled New Haven politics, for using policy formation as the subject matter of data on the distribution of power ignored the possibility that power was also exercised in control of the agenda of political discussion. Thus students of power who limit their observations to policy making "utilize an approach and assumptions which predetermine their conclusions,"[6] since any really dominant ruling group could cow or distract its potential opponents, suppressing controversy or limiting it to trivia. Scholars expressing this viewpoint seem primarily concerned with the problems of establishing the existence of a small ruling group, and so, for purposes of descriptive convenience, I will call them "neo-elitists," adopting the term used by Richard M. Merelman.[7]

Although Bachrach and Baratz's thesis is open to varying ideological coloration, it has been most commonly adapted to the familiar leftist assertion that a covert business elite manipulates American politics in its own interests.[8] But the favorable response to "Two Faces of Power" is by no means ideologically limited.[9] This widely cited and reprinted article poses important problems about research procedures and schemes of analysis for the study of politics. In this article I attempt to explicate Bachrach and Baratz's criteria of research, explore the empirical assumptions on which their recommendations are based, and analyze various ways in which their ideas can be made operational for field research. While some aspects of the idea of nondecisions can be applied empirically (indeed, they have concerned scholars, including Dahl, for some time), other parts of the neo-elitist research agenda are not practicable; and even the feasible aspects of the neo-elitist approach do not provide a sensible basis for statements about the distribution of political power. Short of introducing ideological premises specifying how members of the polity should behave, there is no way to satisfy the neo-elitist critique of contemporary research. As I also hope to show, however, Bachrach and Baratz have helped uncover some of the basic deficiencies of the concept of "power structures" as a focus for field research.

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[1] *The American Political Science Review*, 56 (December, 1962), pp. 947–52.

[2] For a 240-item bibliography on "community power" see Willis D. Hawley and Frederick M. Wirt, eds., *The Search for Community Power* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1968).

[3] Robert A. Dahl, *Who Governs?* (New Haven: Yale University Press, 1961).

[4] Robert A. Dahl, "The Concept of Power," *Behavioral Science*, 2 (July, 1957), pp. 202–203. This definition resembles C. Wright Mills': "By the powerful we mean, of course, those who are able to realize their will, even if others resist it." See *The Power Elite* (New York: Oxford University Press, 1956), p. 9.

[5] Dahl, *Who Governs?* p. 163 (emphasis in original).

[6] "Two Faces of Power," p. 947.

[7] "On the Neo-Elitist Critique of Community Power," *The American Political Science Review*, 62 (June 1968), pp. 451–60.

[8] For examples of their influence on explicitly leftist writers see Todd Gitlin, "Local Pluralism as Theory and Ideology," *Studies on the Left*, 5 (Summer, 1965), pp. 21–45; and Shin'ya Ono, "The Limits of Bourgeois Pluralism," *ibid.*, pp. 46–72.

[9] See, for example, William H. Riker, "Some Ambiguities in the Notion of Power," *The American Political Science Review*, 58 (June, 1964), p. 348. While the idea of nondecisions is frequently invoked in discussions of power, it has had scant impact on empirical research.

Did Dahl Study the Wrong Issues?

The neo-elitist critique begins by questioning the relevance of some of the issues Dahl studied—public education and political nominations—to the proposition that a single social and economic elite dominated New Haven politics: since most of the "Notables" (those who controlled the city's economic resources and high society)[10] lived in the suburbs and/or sent their children to private schools, they were not interested in New Haven public schools or in the parties' nominees for local office; therefore their abstention in these areas is hardly evidence against the proposition that they comprised a ruling elite.[11] The root of the difficulty, Bachrach and Baratz say, is that Dahl's approach "provides no *objective* criteria for distinguishing between 'important' and 'unimportant' issues arising in the political arena."[12]

Bachrach and Baratz did not say in their early articles[13] what properties of an issue should be taken into account in assessing its importance. One solution to this problem was suggested by another neo-elitist, Jan G. Deutsch:

> The primary difficulty with *Who Governs?* as an analysis of the *existing* distribution of power is its assumption that the decisions canvassed represented conflicts sufficiently serious to force *all potentially affected powerholders to mobilize their resources* in the hopes of obtaining a favorable outcome.[14]

This seems to be an excessively stringent criterion for "distinguishing between 'important' and 'unimportant' issues arising in the political arena." If taken literally, this standard would relegate all issues to the "unimportant" category, for when in history have "all potentially affected powerholders" mobilized their resources? Certainly not in, say, the Russian Revolution and Civil War, which were fought with most potential participants on the sidelines. If earthshaking events like these fail to meet Deutsch's standards, what hope can he have of ever finding an important decision in local politics?

In their recent book, *Power and Poverty*, Bachrach and Baratz give a somewhat less demanding measure of importance: "A key issue, in our terms, is one that involves a genuine challenge to the resources of power or authority of those who currently dominate the process by which policy outputs in the system are determined."[15] Recognizing that the consequences of decisions may not be immediately apparent, they suggest that this difficulty may be eased by choosing several issue areas to study, thus giving the researcher time to develop a fuller perspective on outcomes and their implications, and increasing his chances of picking a "key issue." Interestingly, they do not discuss another drawback to their proposal: It involves a conclusion about who dominates politics before data have been collected.[16]

These neo-elitist formulations ignore the substance and costs of policies. Public education was the biggest item in New Haven's municipal budget and powerfully affected many persons' life chances. Moreover, one would think that even a torpid elite would want some control over shaping the next generation's political and social perspectives. Yet the neo-elitists argue that public education is not a good issue with which to test propositions about the distribution of political power because the people who dominate business and high society allegedly do not care about it. How then is decision making in public education to be explained, and just what importance is to be attached to this issue area? Is it reduced to triviality merely because the rich do not care about it?

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[10] By Dahl's criteria there were 231 Social Notables and 238 Economic Notables. Only 24 people were in both groups, which casts some doubt on the neo-elitists' habit of treating the groups as identical (*Who Governs?* pp. 64–68).

[11] There are some elementary factual deficiencies in the neo-elitists' statements: About a third of the Notables lived in New Haven itself. Regardless of where they lived, men qualified as Economic Notables by virtue of their control of property *in the city of New Haven*, which would give them a stake in city politics.

[12] "Two Faces of Power," p. 948 (emphasis in original).

[13] See also, by the same authors, "Decisions and Nondecisions: An Analytical Framework," *The American Political Science Review*, 57 (September, 1963), pp. 632–42.

[14] Jan G. Deutsch, "Neutrality, Legitimacy, and the Supreme Court: Some Intersections Between Law and Political Science," *Stanford Law Review*, 20 (January, 1968), p. 251 (emphasis added).

For a critique of Deutsch's article see Nelson W. Polsby, "On Intersections Between Law and Political Science," *Stanford Law Review*, 21 (November, 1968), pp. 142–51.

[15] Bachrach and Baratz, *Power and Poverty* (New York: Oxford University Press, 1970), pp. 47–48.

[16] The best discussion of the prevalence and deficiencies of this a priori assumption is Nelson W. Polsby, *Community Power and Political Theory* (New Haven: Yale University Press, 1963).

It seems that the neo-elitists have failed to recognize something that strikes many political scientists as a truism: most people are not very interested in politics and choose to spend their time and resources in other pursuits;[17] thus public decisions are made by specialists in politics and those individuals who are interested in the substance of a particular decision. Since, in Dahl's well-known phrase, "minorities rule," the neo-elitists' suggestions and the criticisms on which they are based are likely to lead to research on power that would exclude most policy formation from consideration.

Some neo-elitists (e.g., Deutsch) may accept this line of argument, in the belief that using decisions to study power is fallacious because *no* decisions would elicit maximum resource expenditure by all potential actors, and thus the researcher would never discover the "real" distribution of power.[18] Necessary to this position is the belief that a particular group "could get its way if it wanted to." This assertion is helpful in dealing with inconvenient evidence about the nonparticipation or defeat of putatively powerful groups. In addition to being unverifiable, however, this claim has the disadvantage of being trivial. To say of a group that is could win if it wanted to expend the necessary resources is not to say very much, since often the same could be said of groups on both sides of the same issue. Moreover, some neo-elitists probably believe that "the people," or perhaps just "oppressed minorities," could stage a "revolution" after suitable indoctrination and organization. Could these prospective revolutionaries be any less potentially powerful than the patricians who disdain to participate?

Several critics have said that there was no systematic justification presented for studying education, nominations, and urban renewal in New Haven.[19] But it would be difficult to imagine a set of objective criteria that excluded these issue areas, as long as it was stipulated that we were concerned with how political decisions were made. (This is not to say that these were the only worthwhile issues.) Therefore, while perhaps we cannot specify criteria for choosing decisions to study, we can be sure that some particular sets of decisions are worth studying, while others, being at the boundary of relevance, may not be. If we knew so much about the unequivocally important ones that the only remaining research tasks were at the margins, where importance was uncertain and would be determined in different ways by different criteria, then these criticisms would be useful. But at the present stage of knowledge the problem of criteria of importance is premature.

The Renunciation Form of Nondecision

The third issue we studied in New Haven was the enormous urban renewal program that brought so much attention to the city and to its long-term mayor, Richard C. Lee.[20] One prominent feature of this program was the Citizens Action Commission, whose most significant element was a number of leading businessmen. As Dahl demonstrated, the CAC was a prefabricated pressure group that unvaryingly endorsed the sweeping proposals Lee advanced and then helped win public support for them.[21] Dahl's account of urban renewal politics in New Haven was the point of departure for Bachrach and Baratz's principal contribution, the concept of "nondecisions."

This term was not explicitly defined in "Two Faces of Power": its meaning was indicated by usage and example.[22] These illustrative passages suggest three types of nondecision, each posing somewhat different problems of definition, empirical application, and interpretation. Certain subjects recur in the discussion below, however. One theme common to more than one type of nondecision is "false [class] consciousness," a label for popular opinion that does not follow leftist prescriptions and a shorthand way of saying that "the people" don't know what's good for them. A second common problem is the measurement and assessment of political values, institutions, and procedures, and of people's attitudes toward these rules of the game. A third difficulty—the greatest of all—is what to conclude about the distribution of political power even if particular nondecisions can be identified.

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[17] In "Middletown," for example, the very wealthy "Y" family was wholly uninvolved in civic affairs and maintained a "large degree of isolation from the city's central interests." See Robert S. Lynd and Helen M. Lynd, *Middletown in Transition* (New York: Harcourt, Brace, 1937), p. 91.

[18] This notion may explain the hitherto puzzling distinction that some writers drew between the "power structure" and the outcome of political decision making. For example, Lawrence J. R. Herson mused "*If* power structure does determine community action; and *if* this structure reflects the wishes of top influentials . . ." (emphasis in original). See his "In the Footsteps of Community Power," *The American Political Science Review*, 55 (December, 1961), p. 823.

[19] Polsby developed explicit criteria stating what factors should be taken into account in choosing issues to study (*op. cit.*, pp. 95ff.). Andrew S. McFarland has suggested four additional criteria. See his *Power and Leadership in Pluralist Systems* (Stanford: Stanford University Press, 1969), p. 82. Neither Polsby nor McFarland provided thresholds for distinguishing between important and unimportant issues.

[20] Lee was first elected in 1953 and retired at the end of 1969.

[21] *Who Governs?* ch. 10.

[22] This article and "Decisions and Nondecisions" are included in the recent *Power and Poverty*, which also contains an explicit definition of "nondecision." But in this new formulation the concept has become so

The simplest kind of nondecision is what I will call "renunciation": A political actor—already a participant in policy making—refrains from making a proposal or rejects an alternative among the many which he might consider because he thinks that it will be unacceptable to some person or group. Thus power cannot be adequately studied by observing policy formation and noting who proposes, who opposes, and who wins, because expected vetoes may be more important than actual ones. Bachrach and Baratz applied this idea to urban renewal in New Haven as follows:

> That the CAC did not initiate or veto actual proposals by the mayor was to Dahl evidence enough that the CAC was virtually powerless; it might as plausibly be evidence that the CAC was (in itself or in what it represented) so powerful that Lee ventured nothing it would find worth quarreling with. . . .
>
> How . . . can a judgment be made as to the relative influence of Mayor Lee and the CAC without knowing (through prior study of the political and social views of all concerned) the proposals that Lee did *not* make because he anticipated that they would provoke strenuous opposition and, perhaps, sanctions on the part of the CAC?[23]

I will discuss first the facts of New Haven politics referred to here and then take up the more general problems that these passages raise. Lee originated the CAC and picked its members, beginning with the key appointment, the chairman. It took the mayor nine months to find a businessman who would agree to head the CAC. As in many northeastern cities where politics is the province of ethnic groups while big business remains in the hands of Yankees, big businessmen had largely withdrawn from participation in New Haven politics. Their withdrawal made it all the more difficult for Lee to convince anyone suitable that the city needed massive urban renewal and that he could carry out such a program. One criterion of appointment to the CAC was sympathy with Lee or his policy goals. Some leading businessmen, such as the millionaire who owned both local newspapers and a good deal of downtown real estate, were never considered for appointment because of their hostility to the mayor. Most of the businessmen on the CAC were not supporters of redevelopment before Lee's election. They came to favor and endorse it because he persuaded them to. The inescapable fact of the matter is that Lee took the initiative, convinced many businessmen (and most other local residents) of the desirability of urban renewal, brought the issue to the forefront of public attention, formulated New Haven's urban renewal program, and, using the CAC (as well as many other agencies and techniques), began carrying out that program.[24]

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protean as to lose all utility: "A nondecision, as we define it, is a decision that results in suppression or thwarting of a latent or manifest challenge to the values or interests of the decision-maker" (p. 44). This seems to say that "the decision-maker" perpetrates a nondecision every time he wins.

Subsequent discussion expanded the term's meaning even further: "Simply supporting the established political process tends to have this effect" [of making nondecisions] (p. 50). "An attempt to prevent an issue from reaching the decision-making stage" is a nondecision (p. 57). Examples in the House of Representatives include the Rules and Ways and Means Committees and the seniority system (p. 60). Among the nondecisions uncovered by the authors in their research on Baltimore were: conservative appointments to municipal boards and commissions (p. 70); liberal majorities on municipal task forces (p. 71); and attempts to help the poor by the city's most important business association, for "its warm support for the cause of racial integration amounted to a nondecision in that it helped sustain the ideological breach between the 'bourgeois' and the militant blacks, thereby further weakening the political leverage of the latter" (p. 73).

In view of the expanded meaning suggested by these examples, I have confined my discussion of nondecisions to the earlier articles, except when *Power and Poverty* helps answer questions raised by its authors' previous work.

[23] "Two Faces of Power," p. 952. As many readers may know, Dahl presented a mass of data to support his conclusions about the absence of business dominance of urban renewal in New Haven. The CAC's failure to object to Lee's proposals is only a piece of this proof, not ("evidence enough") the whole argument.

[24] For an extended description of this process of conversion and co-optation see my *The Politics of Progress* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1972), ch. 9. Much of this information was also available to Dahl's critics in *Who Governs?*

The truth about the relationship between the Lee Administration and the CAC seems to have been grasped by the people of New Haven much more readily than by the neo-elitists. By the early 1960's the

New Haven was the 81st largest city in the country in 1960, but during the 1960's it had, in absolute size, the sixth largest urban renewal program.[25] For more than a decade it has had the biggest per capita urban renewal effort in the country, usually two to three times as big as the runner-up city. By the mid-1960's more than 2000 acres in New Haven were in active renewal projects. Even if one believes that Lee never dared propose anything that local big business might dislike, and that therefore an accurate description of New Haven's "power structure" would put big business at the top of the pyramid, the city's urban renewal achievements must still be explained. These accomplishments *could* be the product of the local business community's domination, exercised so covertly that none of us studying New Haven politics could detect it. To accept this proposition, one must also believe that New Haven's businessmen are unique, for their city's urban renewal program has not been equaled anywhere else.

An alternative proposition is that Mayor Lee was the basic cause of the program, but nevertheless was not powerful, because he proposed nothing the businessmen would reject. This latter alternative is wholly consistent with the neo-elitist conception of political power, but, as before, it fails to explain what happens as opposed to what does not happen. To say that Lee did not do this or that because he anticipated opposition leaves untouched the question of the very considerable things he did do, and it was these achievements that seemed the more interesting research topic.

Saying this does not, however, dismiss the problem of anticipated reactions. It is difficult to argue with the thesis that politicians avoid advocating proposals that will meet a cold reception in their constituencies. Like everyone else, they do not willingly or lightly venture where they anticipate hostility, defeat, and perhaps retribution as well. Consider an extreme case: If no politicians dare make suggestions that A (an individual or small group) would oppose, and A is known to oppose any departure from the status quo, then a researcher would be justified in diagnosing a "false consensus" and a ruling elite consisting of A. This seems to be the sort of situation the neo-elitists have in mind when arguing that the study of overt decision making is inadequate. Indeed, the anticipated reactions to which they attach so much weight seem invariably to be those of single individuals or very small groups.

But now consider another kind of anticipated reaction, immeasurably more difficult to interpret in "power structure" terms, but a good deal closer to American political reality: An official is constrained by his expectation of possible adverse reactions from masses of people. Specifically, he fears to advocate policies that he believes may cost votes at the next election. Dahl called this "indirect influence," and it is a key feature in his description of politics in New Haven: "elected leaders keep the real or imagined preferences of constituents constantly in mind in deciding what policies to adopt or reject."[26] This is, of course, the way that elections often serve as mechanisms of popular control. Doubtless this discussion will strike many readers as a bit tedious, if not banal. Yet oddly enough the idea of electoral accountability seems unacceptable to some neo-elitists. While criticizing Dahl's putative failure to consider how Lee may have been constrained by the CAC, Bachrach and Baratz did not cite Dahl's discussion of indirect influence. They did, however, explicitly object to any definition of power that implied that anyone whose behavior affects the outcome of a decision has *some* power:

> But if this is what is meant by power, how can we avoid concluding that no matter where we look, we shall always find that power is broadly diffused? To rephrase, if (a) we analyze the distribution of power solely in terms of decision-making and (b) we ascribe power to all who participate in whatever measure or with whatever "weight" . . . then (c) do we not necessarily

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CAC had acquired a somewhat tarnished reputation as the mayor's creature: "it had served Lee's purposes in urban renewal but in serving them it had lessened its credibility and effectiveness." See Russell D. Murphy, *Political Entrepreneurs and Urban Poverty* (Lexington, Mass., Lexington Press, 1971), p. 39.

[25] The magnitude of urban renewal programs is measured by federal capital grants for all projects in the execution phase, or with a loan and grant contract approved but not formally executed, as of December 31, 1963. The raw data were taken from the *Urban Renewal Directory, December 31, 1963* (Washington: Urban Renewal Administration, 1964). Federal grants generally cover two-thirds of net project costs; the rest is paid for by local funds, in some cases supplemented by state grants. It should not be thought that such projects are "easy." At the end of 1963, 14 years after the beginning of the federal urban renewal program, 29 per cent of cities with populations between 100,000 and 250,000 did not have a single project in the execution phase.

In 1966, on a similar measure of program size, New Haven had a score of $790.25 per capita, compared to a mean of $53.51 for all cities, and was more than eight standard deviations above the nationwide mean. (Source: Robert R. Alford and Michael Aiken, "Community Structure and Mobilization: The Case of Urban Renewal," unpublished paper, University of Wisconsin.)

[26] *Who Governs?* p. 164.

prejudge that power in real-world situations will be widely dispersed?[27]

It is a bit surprising that scholars reject a formulation not on grounds of empirical falsity or impracticability, but because they expect that its implications may be unacceptable.

Although Bachrach and Baratz doubtless are correct in thinking that elections do disperse power somewhat, believing that politicians may be constrained by worries about electoral outcomes is not tantamount to concluding that political influence is evenly distributed. A number of forces can vitiate electoral accountability as a means of indirect influence for some or all of the population. Electoral laws and schedules affect turnout in a selective fashion (see the studies cited in note 60), and categories of people who are less likely to vote will have less indirect influence.[28] Kenneth Prewitt argues that electoral accountability is likely to be restricted when officials do not face effective electoral opposition, retire voluntarily, and lack higher political ambition. (Even unopposed politicians may respond to public opinion, however, either because they think they ought to or to placate opposition before it results in an electoral challenge. For an example of the latter situation, see note 44). Prewitt shows that these conditions are commonplace in the nonpartisan, manager-council suburbs of the San Francisco Bay Area.[29] His data also indicate that city councilmen who are likely to be opposed at the polls are also more responsive to their constituents (or at least say that they are responsive).[30] As these examples indicate, attributing influence to voters whose "real or imagined preferences" may motivate politicians does not necessarily expand the ranks of the powerful indefinitely nor require an assumption that such power is equally held.

The kinds of studies mentioned in the preceding paragraph do not deal directly with manifestations of indirect influence attained by anticipation of electoral reaction. One could learn *something* about how officials are stimulated and constrained in this fashion through participant-observation and interviewing, as we did in New Haven. Such techniques could indicate which groups' electoral displeasure seemed to loom largest in politicians' calculations and which elements were the least cause for alarm or were already written off, and thus had the least indirect influence.[31] We found, for instance, that Mayor Lee made an important change in his urban renewal program for fear that many voters would think him unduly favorable to Yale University, a *bête noire* to New Haven's largely plebeian population.[32]

Doubtless Lee was influenced by many anticipated reactions that we did not know about. How can a researcher get a reasonably complete list of such phenomena while avoiding the problem of infinite regress? Assuming that political actors are aware of the range of alternatives, it would be at least hypothetically useful to ask informants which alternatives they considered, which ones they might have considered and did not, and what community elements they most try to avoid offending. Doing this would produce useful answers, but the responses would be far from exhaustive. There are several problems here: 1) How can the researcher isolate individual decisions as units of analysis so he can identify the suppressed alternatives? Are decisions as discrete as this suggests? 2) How exhaustively will the politicians have pondered the alternatives? Which ones will they consider to be beyond the realm of the practical? Will they recognize the same "real issues" as a New Leftist, old Communist, black nationalist, white supremacist, John Bircher, devout Catholic, vegetarian, etc.? Will they agree on what "objective interests" are being harmed and helped by their decisions? 3) Most politicians would like to avoid offending anyone, yet clearly they have to do so. Should the interviewer specify the stakes that would lead his informant to irritate each actual or potential community group? Would this approach yield valid answers from busy informants who might well grow impatient with such hypothetical questions? 4) Any politician has limited resources and must set priorities, deciding which issue areas will be the principal loci of his time and energy. Would the researcher conclude that all low-priority issue areas represented nondecisions? If so, nondecisions would always be found in all regimes. 5) In view of these considerations, how can any comprehensive attributions of power be made on the basis of the notion of anticipated reactions?

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[27] "Decisions and Nondecisions," pp. 633–34n. Another neo-elitist, Michael Parenti, acknowledged Dahl's discussion of indirect influence, but incorrectly claimed that Dahl limited the term to the poor, and suggested that it was "postulated, without the benefit of empirical research that ordinary voters, exercise indirect controls over decisions . . ." See his "Power and Pluralism: A View From the Botton," *Journal of Politics,* 32 (August, 1970), p. 506. Lee's constant concern with his next election was not postulated, but was ascertained through my direct observation of him and his principal lieutenants.

[28] For a brief discussion of the problem of "translating votes into public policy," see Donald R. Matthews and James W. Prothro, *Negroes and the New Southern Politics* (New York: Harcourt, Brace and World, Inc., 1966), pp. 477–81. William R. Keech has studied the vote as a political resource for Negroes in two southern cities. His findings emphasize limitations on what can be accomplished by means of the ballot box alone and indicate dimensions along which this problem can be analyzed. See his *The Impact of Negro Voting* (Chicago: Rand McNally and Company, 1968).

[29] "Political Ambitions, Volunteerism, and Electoral Accountability," *The American Political Science Review,* 64 (March, 1970), pp. 5–17. For data on the political careers of mayors see Wolfinger, ch. 13.

[30] Prewitt, pp. 11–12.

[31] Whom an official is willing to offend depends in part on how much he can be hurt by a given group, and how much he needs its support. For example, a Democratic Congressman from Chicago need be less solicitous of labor unions than a Democrat elected from a city like Detroit, where party organization is weaker and unions thus are more important politically.

[32] For an account of this episode see Wolfinger, ch. 10.

There is yet another problem: do public officials in fact scan the environment for all possible alternative courses of action? Do they really make decisions according to this "rational-comprehensive" model? Charles E. Lindblom has managed to convince most social scientists that they do not. Instead, officials are more likely to make policy by a model of "successive limited comparisons."[33] The reasons both for recognizing and applauding this model of decision making are neatly summarized by Aaron Wildavsky:

> Organizations would find life unbearable if they treated each stimulus requiring action as something new. Every situation would then require an agreed definition of the situation, a frame of refence for interpreting events, a specification of the mixture of values involved, a thorough search for policy alternatives, and much more. In order to avoid the enormous effort and intellectual capacities required to accomplish these tasks, organizations ordinarily cut their decision costs and their burden of calculation by developing set patterns of responses to frequently encountered stimuli.[34]

It would be difficult, if not impossible, to differentiate renunciation from unconscious limitation of alternatives.[35]

In their earlier articles Bachrach and Baratz did not discuss interviewing of participants as a means of detecting renunciation.[36] Instead, they recommended "prior study of the political and social views of all concerned." But prior to what? Views about what? How can the researcher know prior to the nondecision what views to ascertain in order to find out what has not happened?[37] The context of Bachrach and Baratz's proposal suggests a belief that holistic ideologies provide an adequate basis for predicting attitudes on specific policy questions. The impressive body of research on voting behavior and public opinion that has accumulated over the past dozen years conclusively demonstrates the weakness of this proposition.[38] Most of the New Haven businessmen whom Lee induced to serve on the CAC professed the purest of rugged individualist ideologies to us, yet they were able to reconcile this outlook with enthusiastic endorsement of urban renewal, a new political issue which Lee convinced them was consistent with their "political and social views." Consider the views of one of these men:

> Too much money has gone into the federal government to be disbursed at the capricious whim of a politician. . . . But the way I look at [it] is if taxes are going to remain high and there is going to be a socialist program in the United States . . . if personal income taxes cannot be reduced, why there's only one thing to do and that is to devise ways and means so that we can share in it. . . .

If we examine the problem not from the viewpoint of the politician, but of a group thought to be wielding "non-decision-making power," the problems of devising satisfactory empirical measures are, if anything, even more severe. For just as the object of such power may not be aware of the relationship, the subject may also not know that he is exercising power. Bachrach and Baratz explain the dilemma this way:

> In conceiving of elite domination exclusively in the form of a conscious cabal exercising the power of decision-making and vetoing, he [Dahl] overlooks a more subtle form of domination, one in which those who actually dominate are not conscious of it themselves, simply because their posi-

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[33] The most succinct statement of Lindblom's views on this point is "The Science of 'Muddling Through,'" *Public Administration Review,* 19 (Spring, 1959), pp. 79–88.

[34] "The Analysis of Issue-Contexts in the Study of Decision-Making," *Journal of Politics,* 24 (November, 1962), p. 718.

[35] For an interesting discussion of these "set patterns of responses" see Ira Sharkansky, *The Routines of Politics* (New York: Van Nostrand Reinhold Company, 1970).

[36] In *Power and Poverty* they say (p. 47) that "direct questioning of those concerned will be to little avail" since unawareness, a desire for concealment, or preoccupation with defeat would keep both the subjects and objects of nondecisions from being good informants.

[37] Answers to these questions will not be found in *Power and Poverty,* which reports the authors' field research in Baltimore. If they did follow their own advice about "prior study of the political and social views of all concerned," the results are not reported in this book.

[38] For a creative synthesis of findings on this point see Philip E. Converse, "The Nature of Belief Systems in Mass Publics," in David E. Apter, ed., *Ideology and Discontent* (New York: The Free Press of Glencoe, 1964), pp. 206–61.

tion of dominance has never seriously been challenged.[39]

They are not explicit about the identities of the men whose "position of dominance has never seriously been challenged," although the context broadly hints that they mean big businessmen. In view of the bitter ethnic hostilities that have characterized cities like New Haven, culminating in the Yankees' displacement from public office by the Irish, it is difficult to believe that the Yankees think they have never been seriously challenged. Consider, moreover, the following response by a former president of the Manufacturers' Association of Greater New Haven when Dahl asked him why more businessmen did not participate in local politics:

> I think perhaps—speaking for myself and having observed other people—perhaps we're all scared. Anytime we stick our nose into a political grindstone, we find that for every dollar we can spend, the unions are right there with three times the number of people and three times the number of dollars.
>
> Ever since the New Deal, why, I feel that the businessman has been down the ladder of political influence and I think that it has unduly scared us. I've heard some of these old timers like ——— say that we're a bunch of cream puffs in this political thing and I think maybe he's right. I think we have seen the terrific growth of union power in politics and we have seen what happens. Well, take my own case, for example: two years ago, I went up to Hartford and spoke on behalf of a right-to-work bill at the legislature. I think there were only three other industrialists in the entire state that at that time would stand up and speak on behalf of it while there were five hundred union guys there, including Monsignor ——— and a few others who were very avidly against it. Now, I went up again this time, but I heard through our personnel director that the president of the union put a lot of pressure on him to try to get me not to go up there. He just called Jack and said, "Is that [obscenity] president of yours going up to Hartford and trying to get this right-to-work?" Jack said, "Yes, I think he is." The union president said, "Why doesn't he wise up and lay off that stuff?"
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> Now for instance, take ——— over at ———. There's a guy who believes every bit as strongly as I do about a right-to-work bill, but he wouldn't dare go up to Hartford. He's got a much stronger union and a much more left-wing union over there, and they're very tightly organized.[40]

For all his complaining about labor domination, this man was active in various kinds of public affairs, including the CAC.

It is not necessary to believe in a serenely unchallenged elite to accept the proposition that men may influence others unconsciously. Those people whose probable opposition causes the politician to refrain from certain proposals need not participate in this act; they can be taken account of without their knowledge. This often is the case when politicians adjust their policies in anticipation of voter response. Indeed, in many instances officials doubtless respond to imagined or exaggerated public opinion. When their careers may be at stake, politicians tend to err on the side of oversensitivity to possible complaints. Since there is no necessary interaction between the hypothesized subject and object of the power relationship, the subject could easily be unaware that he had "been powerful."

If the renunciation form of the non-decision-making process were a wholly conscious phenomenon, we might hope to develop ways of studying it by asking questions. But since it often is not, the most promising operational approach could be by means of deductive models.[41] I will examine this possibility after discussing the other forms of nondecision.

The Abstention Form of Nondecision

Renunciation is abjuring of some alternatives by political actors. It is akin to "abstention," which is a conscious decision not to become involved, not to spend resources in a given political transaction. The difference is that the abjured behavior is not the advocacy of certain possibly indeterminate policy alternatives by already active individuals, but rather a decision not to participate at all by people who have grievances or goals that might be met by political action.

The distinction has some point. With renunciation the practical task of locating informants is eased, because the nondecision is "made" by someone who is already active in politics and therefore relatively visible to the researcher. But anyone can be an abstainer, and thus the researcher's universe is the entire polity. Another difference is that with renunciation the researcher does not have the problem of distinguishing between a nondecision and a preference for spending one's time in activities other than politics. But how can this distinction be drawn so as to identify genuine cases of abstention?

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[39] "Two Faces of Power," p. 952n.

[40] Using the reputational method in Seattle, Delbert C. Miller identified a "power structure" of big businessmen. Miller reported that the most important issue decided in Seattle during his research there was a proposed right-to-work law. Most of his "Key Influentials" and "Top Influentials" favored the proposal, but were afraid to express their support publicly. The measure was defeated. See his "The Measurement of Influence in Community Affairs," *Proceedings of the Pacific Sociological Society,* Research Studies, State College of Washington, 25 (June, 1957), pp. 148–54.

[41] McFarland (p. 86) suggests that comparative analysis might identify nonevents in a particular locality.

The importance of this question may be seen by examining how the neo-elitists have discussed abstention. One example they cite is "Springdale," an upper New York State hamlet described in Arthur J. Vidich and Joseph Bensman's well-known book, *Small Town in Mass Society*.[42] The village government, elected by little more than a dozen voters, was controlled by a few local businessmen dedicated to minimal public expenditures. Many residents of "Springdale" disliked their government's refusal to provide more adequate community facilities, but they did not even vote, much less organize an opposition slate of candidates for the village board, which was always elected without challenge.[43] Vidich and Bensman attribute the villagers' abstention from electoral politics to "the complexity of organizing political support; the necessity for historical, legal and technical knowledge in defining an issue clearly; the lack of knowledge of procedure."[44]

These abstainers in "Springdale" may seem very much like the abstaining Notables in New Haven. Yet the neo-elitists draw different conclusions from the two examples. If the "Springdale" abstainers prefer their leisure to community facilities and thus stay out of local politics, they are the victims of power. If the Notables prefer other activities to participation in public education and local politics, then these issues are beneath them and do not provide a basis for any judgments about their power. Clearly, the neo-elitists have not told us enough about how to distinguish between the two cases. What is needed is a means of identifying issues, a problem to which much of the following discussion will be addressed.

If the "real issues" can be identified in the preferences of the public, gathering data on abstention by large segments of the polity does not present insuperable problems. Vidich and Bensman evidently found out about abstention by unstructured interviewing. A similar approach, supplemented by a conventional sample survey, would give empirical expression to this concept by providing data on the extent to which a city's agenda of political discussion reflected its residents' political preferences and actions. Bachrach and Baratz recommend this strategy in *Power and Poverty*. They distinguish "overt" from "covert" grievances. The former "are those that have already been expressed and have generated an issue within the political system, whereas covert ones are still *outside* the system. The latter, that is, are covert in the sense that they have not been recognized as 'worthy' of public attention . . ."[45] In the terminology I use here, covert grievances are one index of abstention. Bachrach and Baratz report that in Baltimore, "If there are covert grievances, they are so well concealed that we cannot locate them."[46]

But "covert grievances" are only one aspect of abstention, which occurs when individuals or groups do not pursue their preferences in the political arena. How does abstention differ from apathy, laziness, pessimism, or lack of interest in politics? These are all widespread; indeed, they are the common condition. Most people prefer spending their time, money, and skill for purposes other than shaping public policy. Moreover, the composition of the activist fraction of the population shifts considerably from electoral to nonelectoral politics.[47] There is also a remarkably high rate of turnover in the ranks of campaign activists from one national election to the next.[48] As with voting, various institutional arrangements and incentive systems encourage or discourage political participation, and it is generally assumed that higher participation works to the advantage of those people whose inclinations toward activity are weakest, *e.g.*, the poor. Such alternative arrangements are what E. E. Schattschneider called "mobilizations of bias," which he defined thus:

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[42] Princeton: Princeton University Press, 1958.

[43] Vidich and Bensman, *Small Town*, pp. 112–23.

[44] *Small Town*, p. 123. Vidich and Bensman go on to describe the anti-status quo activities of the Community Club, which was a means of prodding the village board to act. When the proponents of a specific measure mobilized enough support for it, the board responded favorably in order to avoid organization of an alternative slate of candidates who would threaten the tenure of the incumbent board members (*ibid.*, pp. 128–36). In other words, the village board was moved by anticipated reactions.

[45] *Power and Poverty*, p. 49 (emphasis in original). Parenti (p. 523) suggested a similar way to locate nondecisions: "Any widely felt deprivation discovered by the investigator that fails to become an issue because the deprived don't have the ability to force a confrontation may be considered a 'non-issue.' " But Parenti did not pursue this point in his report of his research on Newark, which is limited to description of several controversies between the city administration and a tiny black neighborhood group organized by members (presumably white) of Students for a Democratic Society. During the thirty months of the organizing campaign, after which the group dissolved, "as many as 150 residents" participated in one way or another in all its activities (Parenti, p. 509).

[46] *Power and Poverty*, p. 96. This sentence is virtually the only mention by Bachrach and Baratz of their findings from the sample survey of Baltimore residents, which they apparently did not rely on to discuss "overt grievances."

[47] See Dahl, *Who Governs?* ch. 26; and Wolfinger, ch. 5.

[48] Carolyn Ban, "The Party Activist, 1956–68" (Stanford: Stanford University unpublished doctoral dissertation, 1972).

> All forms of political organization have a bias in favor of the exploitation of some kinds of conflict and the suppression of others because *organization is the mobilization of bias*. Some issues are organized into politics while others are organized out.[49]

Specific examples of mobilization of bias have been studied at some length and are discussed in the following section. But it must be recognized that under any circumstances relatively few people will devote much time or money to political activity. Doubtless most readers of this *Review* have grievances which they have failed to remedy by means of political participation. In this sense, then, nondecisions will always be prevalent, for it is difficult to imagine a society populated by people pursuing their policy preferences through political activity.[50] Since we cannot define "abstention" so as to exclude this situation, what conclusions about a city's "power structure" can be based on the concept?

Abstention can characterize organizations as well as individuals, and this poses some knotty research problems. Consider, for example, the problem of a political scientist writing a case study on the adoption of a project to redevelop a city's central business district. He would certainly explore the background, character, and consequences of various interest groups' involvement in the decision. If he noticed that the local Chamber of Commerce did not take a stand, he might try to find out why an organization representing local merchants had nothing to do with an issue of such apparent importance to its members. Would he also inquire into the reasons why the Knights of Columbus or the Junior League did not participate? Probably not. Why not? Because he entertained some implicit assumptions about the rational behavior of the city's interest groups which led him to expect the Chamber to participate and did not suggest to him that the other groups "should" do so. In other words, the researcher had a rudimentary theory about the rational behavior of interest groups.[51] This theory would lead him to cases of abstention as opposed to simple absence of activity. It would also lead to a dilemma, but consideration of this should be postponed until the third type of non–decision making has been mentioned.

The Nonparticipation Form of Nondecision

I will call this third form "nonparticipation": situations in which people are unaware of their interests and therefore do not demand that those interests be served. This unconscious non–decision making is said to be due to values and procedures that set limits on the community agenda of issues. How can examples of nonparticipation be isolated for purposes of empirical study? One approach would be to look at those who are said to exercise power in such cases. Bachrach and Baratz define them as follows:

> . . . power is also exercised when A devotes his energies to creating or reinforcing social and political values and institutional practices that limit the scope of the political process to public consideration of only those issues which are comparatively innocuous to A. To the extent that A succeeds in doing this, B is prevented, for all practical purposes, from bringing to the fore any issues that might in their resolution be seriously detrimental to A's set of preferences.[52]

They amplify these ideas in their own definition of "mobilization of bias": "a set of predominant values, beliefs, rituals, and institutional procedures ('rules of the game') that operate *systematically and consistently* to the benefit of certain persons and groups at the expense of others."[53] The polity seems divided into "persons and groups seeking reallocation of values" and "persons and groups committed to existing values."[54]

Bachrach and Baratz have not offered evidence about the distribution of any relevant attitudes among the population, either from their own work or that of others, nor have they specified which beliefs and procedures allegedly are biased in favor of "the elite."[55] But from the above and other passages one concludes that

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[49] E. E. Schattschneider, *The Semi-Sovereign People* (New York: Holt, Rinehart and Winston, 1960), p. 71.

[50] Mancur Olson, Jr. has argued that abstention from collective political action may be rational behavior for any individual, since the likelihood that his own action will produce benefits to him is minimal. See *The Logic of Collective Action* (Cambridge: Harvard University Press, 1965).

[51] In Bachrach and Baratz, *Power and Poverty* (pp. 140–64) James R. Taylor's appendix on "Key Actors in Baltimore's Anti-Poverty Public" does not consider unions, political machines, civil rights groups, neighborhood organizations, or federal field offices; nor is there any discussion of why they were excluded. One assumes that some explicit or implicit theory led to these omissions.

[52] "Two Faces of Power," p. 948.

[53] *Power and Poverty*, p. 43 (emphasis added).

[54] *Ibid.*, pp. 53, 56.

[55] Cf. Merelman, pp. 453–54. There is no indication in *Power and Poverty* that the authors used their sample survey or unstructured interviewing of political actors to gather data on attitudes toward values, institutions, or procedures.

they envision a single "mobilization of bias," which in turn is accepted by and benefits a unified segment of the population. The attitudinal components of the mobilization evidently are thought to be an internally consistent, unidimensional body of beliefs ("prevailing norms" or "the dominant view").[56] Political procedures and institutions likewise are harmonious, result from the same sources, and have common effects on the fortunes of the various elements in the polity. Policy outputs thus also have a consistent, class-related impact on the population.

Now these propositions are, in varying degree, amenable to empirical investigation. Social scientists have done a good deal of research on some of them; their findings generally contradict the neo-elitist assumptions. In particular, public support for prevailing political values and institutions has been studied in some detail, and a fair amount is known about the incidence of these attitudes, interrelationships among them, and the characteristics of people holding them. The research of Herbert McClosky and Jack Dennis reveals mixed and inconsistent attitudes toward the values and practices of American politics, with favorable attitudes toward some features coexisting with negative views toward others.[57] For example, Dennis describes "a somewhat ambivalent and irresolute state of diffuse support for the party system. Predominantly favorable perceptions and norms are mixed with unfavorable ones."[58] McClosky reports similar patterns in attitudes toward a variety of political values and rules of the game. Although they were by no means united, his elite respondents (national party convention delegates) supported these values and practices more consistently than did the general public. This might provide some confirmation for the neo-elitists, except that the items in question run largely to presumably desirable things like due process, tolerance of dissent, trust in freedom, and so forth.

It is difficult to see how some values and procedures, such as free speech and popular elections, "operate systematically and consistently to the benefit of certain persons and groups at the expense of others."[59] Other practices are biased in favor of particular political interests yet are found in combination with procedures that work in the opposite direction. For example, electing city councilmen from wards helps geographically concentrated interests like minority groups, while electing them on a nonpartisan ballot reduces the turnout of uneducated voters and thus the potential influence of members of minority groups.[60] Sixty-seven American cities with 1960 populations of more than 50,000 elect their councilmen predominantly from wards; 32 of these elect them on partisan ballots and 35 do it on nonpartisan ballots.[61] Clearly, the "mobilization of bias" in the first group of cities is more consistent than in the second group.

These data cast doubt on the universality of the neo-elitists' assumptions about the distribution of opinions, procedures, and institutions, but they do not as such discredit the idea expressed in the passage cited in note 52. If we could assume that the putatively powerful were fully aware of the nature and import of their own behavior in regard to controlling the agenda, that is, if "A" knew what he was doing and what the consequences were, then perhaps a researcher could ask likely candidates about their behavior in this respect. But a page after the passage about "A" and "B" quoted above, Bachrach and Baratz, explicating the "central point" of their argument, greatly reduced its operational utility: "to the extent that a person or group—*consciously or unconsciously*—creates or reinforces barriers to the public airing of policy conflicts, that person or group has power."[62]

Let us take this statement literally and imagine a town where the rich have brought about widespread public acceptance of nonpartisan elections for local office, thereby reducing disproportionately the turnout of the working class and increasing the electoral chances of Republican city officials who will resist demands to change the status quo. If the working class endorses this barrier to "the public airing of policy conflicts," it must, by the definition just quoted, be powerful. Clearly, a better method for identifying the powerful is needed.

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[56] *Power and Poverty*, p. 58.

[57] See, e.g., Herbert McClosky, "Consensus and Ideology in American Politics," *The American Political Science Review*, 58 (March, 1964), pp. 361–82; and Jack Dennis, "Support for the Party System by the Mass Public," *The American Political Science Review*, 60 (September, 1966), pp. 600–15.

[58] Dennis, p. 604.

[59] Procedures that increase popular participation in decision making may also increase the probability that issues will be decided so as to maintain social or political inequality. For a discussion of this problem see Raymond E. Wolfinger and Fred I. Greenstein, "The Repeal of Fair Housing in California: An Analysis of Referendum Voting," *The American Political Science Review*, 62 (September, 1968), pp. 767–69.

[60] For data on the effect of nonpartisan ballots on the turnout of various groups see Robert H. Salisbury and Gordon Black, "Class and Party in Partisan and Non-Partisan Elections: The Case of Des Moines," *The American Political Science Review*, 57 (September, 1963), pp. 584–92; Eugene C. Lee, *The Politics of Nonpartisanship* (Berkeley and Los Angeles: University of California Press, 1960); and Robert R. Alford and Eugene C. Lee, "Voting Turnout in American Cities," *The American Political Science Review*, 62 (September, 1968), pp. 796–813.

[61] The "inconsistency" of municipal governmental forms is discussed in Raymond E. Wolfinger and John Osgood Field, "Political Ethos and the Structure of City Government," *The American Political Science Review*, 60 (June, 1966), pp. 312–15.

[62] "Two Faces of Power," p. 949 (emphasis added).

Bachrach and Baratz have made a suggestion along these lines in the often-quoted peroration of their first article:

> Then, having analyzed the dominant values, the myths and the established political procedures and rules of the game, he [the researcher] would make a careful inquiry into which persons or groups, if any, gain from the existing bias and which, if any, are handicapped by it. Next, he would investigate the dynamics of *nondecision-making;* that is, he would examine the extent to which, and the manner in which the *status quo* oriented persons and groups influence those community values and those political institutions . . . which tend to limit the scope of actual decision-making to "safe" issues. Finally, using his knowledge of the restrictive face of power as a foundation for analysis and as a standard for distinguishing between "key" and "routine" political decisions, the researcher would, after the manner of the pluralists, analyze participation in decision-making of concrete issues.[63]

At the end of *Power and Poverty* they indicate the way to begin such a program: "we ask neither 'Who runs things here?' nor 'Does anyone run things here?' but rather 'Is the distribution of benefits and privileges highly unequal and, if so, why?' "[64]

This seems to equate "who governs?" with "who benefits?" on the assumption that those who profit from what the government does or fails to do have brought about the policies in question, by direct control of officials or by more subtle and fundamental influence over the substance of public discussion. This proposition can be verified; indeed, many social scientists think that we already know enough to reject it, for the following reasons: The consequences of political action often are not what the actors intended. Many people benefit from policies which they vigorously opposed—doctors and Medicare come immediately to mind. In addition to achieving the goals of those responsible for them, public policy decisions often provide side benefits to people who have had nothing to do with their resolution.[65] Although the Black Panther party opposes gun registration and has benefited from the weakness of present laws, its opposition was not responsible for congressional refusal to enact effective gun control legislation in 1968.

It is also true, however, that identifying those who gain and lose from a decision often provides useful clues to understanding how that decision was made. There is a substantial and growing body of research on policy outputs, much of it pertinent to the ideas expressed by the neo-elitists.[66] This research is based on the wide range of variation among American states and cities with respect both to policies and to the assorted institutions and procedures that comprise "mobilizations of bias." It is astonishing that the neo-elitists have neither used this literature nor done any similar research of their own to specify and validate their general allegations about the policy consequences of alternative ways of organizing political life. One explanation of this situation may be that the neo-elitists consider these differences trivial. Such a viewpoint is difficult to accept, however, for the variations in policy outputs among American governmental jurisdictions are very large, as is the range of municipal and state governmental forms and procedures.[67]

Just as the neo-elitists assume that the "mobilization of bias" is unitary and internally consistent, so they seem to attribute unidimensionality to the distribution of outputs. In point of fact, however, the extent to which different social groups profit from government programs varies enormously along several dimensions. One is the type of program: some kinds of spending provide disproportionate gains to the very poor, some to the modestly self-sufficient, some to the middle classes, and some to the rich. The same is true of tax burdens: some taxes fall most heavily on the rich, some on the poor, and so on.[68] It appears that, at least at the state level, there is not always consistency in the size of programs that benefit particular social classes; that is, state policies do not display a consistent class bias.[69]

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[63] *Ibid.,* p. 952 (emphasis in original). Deutsch's recommended strategy is equally demanding: ". . . we may conclude that any study of the distribution of political power that does not include a detailed analysis of the origins and content of the community agenda must be adjudged incomplete" (p. 256).

[64] *Power and Poverty,* p. 106. As a matter of fact, nowhere in this book is there even a fragmentary description of the distribution of public benefits and burdens in Baltimore. As with Parenti (pp. 507–508), who also criticized "pluralists" for focusing on processes as opposed to outputs, Bachrach and Baratz did not present data on who benefits. Here, as elsewhere, they failed to follow their own advice on how to do research on power. It is difficult to see how their empirical material differs from conventional narratives of political controversy and decision.

[65] For data on these propositions see Wolfinger, ch. 1. See also Polsby, *Community Power and Political Theory,* pp. 132–36.

[66] Citations of many of these studies may be found in Brian R. Fry and Richard F. Winters, "The Politics of Redistribution," *The American Political Science Review,* 64 (June, 1970), pp. 508–22; and Philip B. Coulter, "Comparative Community Politics and Public Policy," *Polity,* 3 (Fall, 1970), pp. 22–43.

[67] For data on output variations see, *e.g.,* Fry and Winters, p. 515; and Sharkansky, ch. 6. Wolfinger and Field describe variations in the incidence and combinations of local governmental forms.

One could aggregate, for each income category, all benefits and revenues to obtain an index of the net redistributive impact of each city's or state's policies. Brian R. Fry and Richard F. Winters have computed such redistributive ratios for 48 states.[70] These scores provide a basis for assessing the net redistributive effect of state policy.[71] The ratio of benefits to burdens is greatest for the poor. In all states the redistributive ratio for those families with annual incomes of $4000 or less is clearly favorable, *i.e.*, they get more than they give. These ratios vary enormously from state to state; the most progressive ratio for the poor, in Massachusetts, is 3.32, compared to 1.62 in the most regressive state, Virginia.

One would not conclude from these data that because poor people are net gainers in state politics they are more powerful than social classes with a net deficit. Nor would one conclude that the poor are twice as powerful in Massachusetts as in Virginia. Among the many reasons for resisting such foolish conclusions would be one that probably is most appealing to neo-elitists: attention should be paid to what the government fails to do as well as to what it does, and it could intervene in the private sector to provide the poor with a *fair share* of the wealth. But what is a fair share? Is it necessary to answer this question before doing satisfactory research on power?

This question may be sidestepped by further reference to the policy output literature. Fry and Winters report that interstate variations in the redistributive ratio are more strongly associated with political than socioeconomic variables, a conclusion pointing the way toward research that would specify those political features most beneficial to various elements of the polity. Systematic empirical attempts to explore this problem have, of course, been going on for some time.[72] On the basis of the research reported to date, one can note some important refinements in conceptualizing this problem: Policy outputs that are harmful to particular social groupings may not necessarily result from political arrangements that are disadvantageous to those groupings. Thus Richard I. Hofferbert found no relationship between state legislative malapportionment and the level of welfare oriented policies. A weak negative relationship between state aid to cities and malapportionment disappeared when southern cities were excluded from the analysis.[73] Also at the state level, Ira Sharkansky found, in areas where the quality of public services could be measured independently of the amount of money spent for the services, that there was no necessary relationship between the two.[74]

Structural and procedural variations are probably greater among cities than states, which has given rise to a good deal of speculation about the ways in which alternative governmental forms enhance or restrict the political influence of various groups. Systematic research on this problem is still in its infancy. One problem in such research is obtaining a valid measure of popular demand that can be related to a particular type of output, in order then to determine if such relationships are greater under some institutional arrangements than under others.[75] One way to solve this problem is to take need as an index of demand. This is the path followed by Michael Aiken and Robert R. Alford in their study of relationships between the amount of public housing in American cities and (among others things) municipal governmental forms. When they controlled for the size of cities and the proportion of dilapidated housing, they found a modest but significant negative correlation (−.16) between the number of public housing units per capita and the presence of the city manager form of government. Likewise, they found a smaller but still significant negative association (−.09) between public housing and nonpartisan elections.[76] These findings thus furnish some empirical support for the conventional wisdom that both city manager government and nonpartisan ballots tend to reduce the political power of the poor, who are the beneficiaries of public housing.

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[68] For specific examples of these commonplace observations see Fry and Winters, pp. 514–15.

[69] Sharkansky, *Routines of Politics*, pp. 92–93.

[70] Fry and Winters, p. 515.

[71] See Fry and Winters, pp. 511–15 for various cautions about the data.

[72] A milestone in this field is Richard E. Dawson and James A. Robinson, "Inter-Party Competition, Economic Variables and Welfare Policies in the American States," *Journal of Politics*, 25 (May, 1963), pp. 265–89.

[73] Richard I. Hofferbert, "The Relation between Public Policy and Some Structural and Environmental Variables in the American States," *The American Political Science Review*, 60 (March, 1966), pp. 75, 77. Other studies have produced generally similar results. For a summary of research on this subject, see John G. Grumm, "The Effects of Legislative Structure on Legislative Performance," in Richard I. Hofferbert and Ira Sharkansky, eds., *State and Urban Politics* (Boston: Little, Brown and Company, 1971), pp. 302–305. Grumm's article discusses the policy consequences of various kinds of legislative organization, *i.e.*, of some "mobilizations of bias." Like other scholars who have asked "What effects do the structures of political institutions have on the performance of these institutions" (p. 298), he is able to deal with this question without recourse to the concepts of nondecisions and "power structures."

[74] Ira Sharkansky, "Government Expenditures and Public Services in the American States," *The American Political Science Review*, 61 (December, 1967), pp. 1066–77.

[75] For a discussion of failure to deal with these problems in one such study, see Raymond E. Wolfinger and John Osgood Field, "Communications," *The American Political Science Review*, 62 (March, 1968), pp. 227–30.

As I said earlier, it is noteworthy that the neo-elitists have paid no attention to the policy output literature. It is perhaps even more interesting that the scholars working in the field of outputs have gotten by with virtually no discussion either of power generally or of power structures, nondecisions, mobilizations of bias, and the other conceptual contributions of the neo-elitists. Although they appear to be doing what the neo-elitists recommend, they use quite different frames of reference. The earlier "power structure" studies also seemed isolated from other sorts of work on local politics. Students of "community power" seldom discussed their data for purposes other than compiling lists of the powerful, and rarely used or even cited political science literature outside their field. By the same token, scholars working in other aspects of local politics usually eschewed mention of the power structure literature and textbooks on local government customarily treat it in a separate chapter. This mutual segregation was relatively easy to understand, given the special concerns of the power structure scholars, the tiny slice of phenomena encompassed by their work, and the doubts that many political scientists had about it. But the neo-elitists' concerns are much broader than those of Floyd Hunter and his school, and many of the inquiries they recommend seem to be exemplified by the studies I have mentioned in this section. This mutual indifference indicates, I think, that neo-elitist thought is not readily reconciled with empirical research. It is also likely that the neo-elitists would not be satisfied that the policy output scholars were adequately concerned with nondecisions.

For one thing, serious analysis of "the dominant values and the political myths, rituals and institutions which tend to favor the vested interests of one or more groups" requires trying to find out how these groups would fare if *other* values, myths, rituals and institutions were dominant. This analysis would include values, etc. that are beyond the range presently found in the United States. The neo-elitists seem to believe that present values, etc. favor the rich,[77] yet it is by no means obvious that the poor would be better off under any alternative political system. In other words, things can get worse as well as better for almost any group in the country.

The neo-elitists generally look at the political scene from the left, and it seems likely that when they talk about nondecisions they have in mind the consistent failure of the American political agenda to resemble the Marxist or New Left image of the "underlying issues." Yet there is nothing inherently leftist about the idea of nondecisions. It is, in fact, a notion congenial to all "fundamental" critics of the contemporary political scene, regardless of their ideological preferences. The manufacturer quoted earlier who said that his fellow businessmen did not venture into politics because they were "unduly scared" by the power of unions was alluding to his own set of nondecisions. Indeed, followers of the radical right would have little difficulty accepting the abstract ideas in the neo-elitist view of American politics, much as they might differ from Bachrach and Baratz in their nominations of those who "gain from the existing bias."

The eclectic potential of neo-elitist thought may be more apparent now in the aftermath of the Wallace presidential campaign and a series of mayoral elections in which conservative candidates struck a responsive chord in working class areas with the charge that their opponents were "limousine liberals."[78] In these campaigns it was clear that the image of "the Establishment" was of far greater practical use to rightist politicians than it has been to the SDS or the Black Panthers. For example, Samuel Yorty, the conservative Mayor of Los Angeles, based his upset 1969 campaign on his claim to be a "little man" challenging the big interests, typified by the *Los Angeles Times,* which backed his liberal black opponent.

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[76] "Community Structure and Innovation: The Case of Public Housing," *The American Political Science Review*, 64 (September, 1970), p. 859.

[77] *Cf.* Deutsch: "As long as that agenda continues to benefit the elite, therefore—and if an elite exists at all it is presumably because the agenda *does* favor it—it seems reasonable to expect that the elite will remain largely indifferent to the outcomes of community decisions" (p. 255).

[78] For a brief description of the social bases of Wallace's support see Philip E. Converse *et al.*, "Continuity and Change in American Politics," *The American Political Science Review*, 63 (December, 1969), pp. 1102–1103. Some of the mayoral elections are described in James Q. Wilson and Harold R. Wilde, "The Urban Mood," *Commentary*, October, 1969. Andrew Greeley explains this conservative populism this way: "From the perspective of the Polish TV watcher . . . on the northwest side of Chicago, the long-haired militants are every bit as much part of the Establishment as are the presidents of corporations. . . . They see the protesters and the militants as sons and daughters of the well-to-do, who have attended elite colleges and are supported financially by their parents through all their radical activity" ("The War and White Ethnic Groups: Turning Off 'The People,'" *The New Republic*, June 27, 1970, p. 15).

This is *not* to say that the neo-elitists have devised a delusion that fits equally well malcontents of the right and left, but rather that what is a nondecision depends on one's beliefs about how well contemporary political discussion and action match the ultimate political good. At the present stage of American politics some important nondecisions include: who should be allowed to vote, express his opinions without fear of punishment, organize for economic action, or worship openly. These questions have all been sources of considerable agitation at various times in our past, and some Americans are discontented about the ways they have been resolved. But resolved they have been, and they are no longer the subject of public debate. The "established political procedures and rules of the game" with respect to voting undoubtedly are biased toward some groups and against others; the same could be said about various other features of our political system that represent a departure from, say, Portugal. For this reason, then, the direction and extent of the present system's "mobilization of bias" may not be so easy to determine; or, to put the same point differently, which way it is adjudged biased depends on the direction one thinks it is from the ideal polity.

The neo-elitists also recommend a look at the *origins* of values, institutions, and agenda, maintaining that any study of power in a city that did not take up these questions would be incomplete, and that these researches should precede study of "decision-making of concrete issues." Here again one finds the problems of research procedures and of the conclusions about power he can draw from the results. On the first question: Serious study of the origins of American political values and institutions would take one on an exercise in the history of political thought. How far does one go? To Charles Beard, *The Federalist,* Montesquieu? And then, presumably, attention should also be given to contemporary mechanisms of political socialization by which the young develop their political values as well as their perceptions of political processes and expectations of their own likely success or failure in political action. One instrument of political socialization is the public school system. Is it not possible that in ignoring this subject, the New Haven patricians are at least as mistaken about their real interests as the Newark blacks who ignored the SDS? It may be wrong to think that false consciousness, like pellagra, is an affliction limited to the poor.

It can hardly be disputed that the development of political thought and institutions, the political socialization of the young, political interactions in the family, control of the school system, and similar topics are worthy objects of study. But what can be made of contemporary power relationships in all of this? Do we attribute power to John Dewey for his continuing influence in American education? How should power be apportioned between those interests responsible for adoption of a particular procedure and those which seem to benefit from its operation? In some cases, such as the initiative and referendum, the causes and the beneficiaries are very different. When one considers the possible combinations of existing and alternative versions of institutions, beliefs, and procedures, it becomes clear that the neo-elitists are urging not so much a plan of research as a counsel of perfection.

Criteria for the Study of Nondecisions

Bachrach and Baratz's research prescription is an impossibly tall order for any conceivable number of well-financed scholars. Can the idea of nondecisions be made a useful, if less global, topic for research? The answer, I think, is still negative. The core of the problem is the difficulty of identifying nondecisions, which seems generally to come back to determining people's "real interests," as opposed to what they say they want or what they are trying to get through political action. As with the abstaining Chamber of Commerce in the previous section, some examples of false consciousness are indisputable, *e.g.*, the long period of feeble protest by southern Negroes. Their reticence was due in part to repression, but much of it was voluntary, based on myths and procedures, such as the one-party system, that minimized civil rights on the southern political agenda.[79] Almost any social scientist would agree that the blacks had been manipulated, because almost any social scientist's views of rational political behavior, irrespective of their specific character, would attribute certain goals to southern Negroes.

The difficulty comes when one moves beyond indisputable cases like this one. While they have made no explicit statements on this point, the neo-elitists seem to have a far wider range of issues in mind. What criteria can be developed to guide the researcher to nondecisions *before* they become part of the political agenda? Specifically, how can bona fide examples of nonparticipation and abstention be distinguished from the common apathy and lack of public spirit that characterize political life? The student of political power cannot be expected to concern himself with every event that might happen but has not yet come to pass; he needs guidelines to know *which* nonevents to study. These rules would have to meet two standards: 1) They would not rely on *ex post facto* detection based on an increase in political action subsequent to the period under study. 2) Some threshold for the passage from nondecision to a place on the agenda would have to be established.

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[79] See, *e.g.*, V. O. Key, Jr., *Southern Politics* (New York: Alfred A. Knopf, 1949); and Matthews and Prothro.

The relevance of these two standards can be illustrated by referring to an argument that is sometimes made in support of the idea of nondecisions: Forty years ago most whites were unconcerned about racial discrimination, although conditions were dreadful; the present salience of the issue demonstrates the significance of this nondecision at that time. But was discrimination really a nondecision in the 1930's? At least some Negroes were discontented and tried to do something about it. While the civil rights issue was not as salient in the 'thirties as it became two decades later, it can hardly be said that there were no interest groups concerned with racial discrimination, that politicians did not worry about it, that no litigation concerned it, that people did not talk about it, or that numerous scholarly and popular books did not discuss it. At what point along the continuum of visibility from the pole of unconscious but "real" interests to the pole of daily front page coverage does an issue area stop being a nondecision?

This problem is less basic, however, than the question of how nondecisions can be detected before they cross the line. If we have to wait until after the fact to recognize a nondecision, criticizing Dahl for not studying nondecisions comes close to taxing him for not predicting the future.[80] Consider the following contemporary manifestations of intense political demands: black militancy, student power, the John Birch Society, police militancy. None of these was visible fifteen years ago; were they all nondecisions then? Consider some possible future developments: employers refuse to deduct social security contributions from their employees' paychecks; financial institutions refuse to buy or assist in the sale of municipal bonds; thousands of enlisted men ordered overseas stage sit-ins on the docks; most white municipal employees walk out when a black is elected Mayor of Detroit. Are these nondecisions also? Very similar things have happened some place or another in the modern world. Obviously one could think up hundreds of examples like these. If they are nondecisions, where does the hunt for nondecisions stop? If they are not nondecisions, how do we know it? The infinite variety of possible nondecisions also reveals the idea's adaptability to various ideological perspectives. Moreover, the problem of ascertaining anything about the distribution of power becomes overwhelming: with so many nondecisions, almost anybody—or nobody—can be labelled powerful.

Answers to the questions in the preceding paragraph might lie in an alternate approach which was suggested earlier, most strongly in the hypothetical case of the abstaining Chamber of Commerce and Junior League. We need a theory of political interests and rational behavior that would state when various groups and classes of individuals "should" pursue given goals, so that we could isolate instances where they had failed to do so and thus identify cases of nonparticipation. Yet there is a flaw in this approach: a group's failure to act could be either an example of nonparticipation *or* evidence that the theory was wrong; there is no way to distinguish between the two alternative interpretations.[81] Since different groups' "objective" interests are often far from self-evident, identification of nondecisions by means of a normative theory would be satisfactory only to those people who shared the ideological perspective from which the attribution of interests was made. This conclusion might not be repugnant to some advocates of nondecisions,[82] but it should give pause to those among us who prefer an empirical political science in which the acceptability of a scholar's findings is not determined by the political preferences of his readers.

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[80] No one seems to have found this failing in the work of those scholars who report cities dominated by small elites, although one would think that their work was equally vulnerable to the charge.

[81] I am indebted to Lawrence E. Rose and Richard F. Winters for this point.

[82] Cf. this comment by Ono (p. 52):

> As a general model of politics in American society, the ["pluralist"] theory just presented cannot be challenged on a purely empirical basis. It is an ingenious theory. To be sure we can point out certain tendencies in American society that seem to contradict its optimistic conclusions, but as long as we remain within the framework of purely "empirical" and "descriptive" analysis, we can never clarify the fundamental limitations of this general conception.

The foregoing is not a denial that the political agenda is constrained by a variety of factors including political socialization, ideology, procedures, and institutions. The origins of all these things and their relationship to the contemporary behavior of individuals and governments are worthy subjects for scholarship. It may also be useful to study what governments fail to do, as well as what they do do. One approach to this study might be an examination of things that some governments do and others do not, which could lead to explaining either the positive or the negative. Another approach could be to think up things that no domestic governments do and then try to explain why not. None of these topics, however, necessarily involves attributing political power to various people. Moreover, while sensitivity to all these questions is highly desirable, no conceivable research project could cover the possible nondecisions, and so the final product would still be vulnerable to the kinds of criticisms the neo-elitists have made of *Who Governs?* Their critique, then, should not be reserved just for "pluralists" or those who study power through observation of policy formation, but instead should be applied to all empirical political scientists.

A Plea for Modesty

This discussion has ranged so far afield that it may be useful to summarize some of the main points of my argument about nondecisions and research on power. Clearly, the general issues discussed by Bachrach and Baratz are important to the study of politics. But in several important respects the criteria they propose require data that are difficult to gather or largely unobtainable. Examples of such impracticable requirements include: (1) measuring politicians' anticipations of responses to alternative courses of action; (2) defining abstention in such a way as to distinguish it from apathy or unwillingness to participate; (3) assessing the impact on participation and on the distribution of governmental benefits of alternative values, procedures, and institutions; (4) identifying those responsible for contemporary values, procedures, and institutions.

Equally formidable objections concern how one draws sensible conclusions about the distribution of political power from the sorts of data described in the preceding paragraph. In part this reflects contradictory criteria: (1) those who are responsible for a particular institution or procedure may not be its beneficiaries; (2) factors that increase participation in politics may not necessarily also help equalize the distribution of public goods. More important, there seems to be no way to develop a common currency of units of power from all the nondecisions that research would turn up. For example, how does one deduce power from a politician's sensitivity to the electorate and then express the result in terms that can be aggregated with the power deduced from, say, an understanding of the etiology and consequences of the nonpartisan ballot?

Even when data can be collected on some aspects of nondecisions, only a partial and perhaps highly skewed picture of "the power structure" would be obtained, and the result would still be open to the charge that all aspects of the "non-decision-making process" had not been studied. Explaining the attitudinal and institutional context in which decision making occurs is a desirable goal, but if it is a prerequisite to studying policy formation, no one would ever get to the latter topic.

While Bachrach and Baratz's prescription for research is impracticable, their critique does help point up the inherent futility of trying to discover "community power structures." Before discussing this point, I want to touch on one subject of controversy that can be clarified by a redefinition. One source of complexity in discussing power is the familiar definition of power as capacity: "A has power over B to the extent that he *can* get B to do something . . ." Thus if one entertains a proposition that a particular group has predominant power, it is impossible to disconfirm the proposition with data on the outcome of decisions, for its proponents can always argue that the decisions studied are unimportant to that group (however important they may be to others) and that *if* its main interests are threatened, the putative elite *can* get its way. Such threats are uncommon in American politics, hence the proposition is very difficult to disprove. But the invulnerability to disproof of *all* such allegations means that a variety of propositions nominating different groups as "the elite" would be equally respectable under the criterion of power as capacity. Since defining power as a capacity to get one's way seems to lead to essentially unverifiable claims about potential power, it would be simpler to think of power as a relationship, which would produce a definition like this: power is a relationship in which A gets B to do something that B would not otherwise do. This would reduce the relevance of the "they could if they wanted to" syndrome that complicates discussions of previous research.[83]

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[83] For a similar definition see Dahl, *Modern Political Analysis*, Second Ed. (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1970), p. 17.

With this problem out of the way, one could then investigate such propositions as: (1) Unions are more likely than business groups to get their way on issues of type X, but less likely to win on issues of type Y. (2) Group K is more effective in one arena than in another, *e.g.*, blacks are more likely to succeed in legislatures than in referenda. (3) A given resource is more important at one stage of the legislative process than another, *e.g.*, congressional seniority is more important in appropriations than in original authorization of pork barrel projects. (4) The visibility of certain issues affects the power of the contenders, *e.g.*, as public discussion and knowledge of a consumer issue increase, so do the chances for victory of consumer groups as opposed to producers. (5) Higher turnout in municipal elections is likely to be related to greater success for candidates supported by groups A, B, and C, and less success for candidates supported by groups D and E.

These are rather limited kinds of statements, and *they can all be phrased so as to avoid the term "power" and its synonyms.* But to talk about "power structures" is to raise a subject that is inherently comprehensive. The term "structure" connotes, if not exhaustive completeness, at least a description of all the principal elements of a system. The system might be limited to a single issue area, or it might encompass all political decisions within a given governmental jurisdiction. Attempting to describe a power structure also presupposes that one can attribute power to actors so that the power of different actors can be compared, and so that their power can be aggregated in order to estimate the relative power of different coalitions.

In trying to meet these criteria the researcher confronts at least three difficulties which cannot be surmounted in any way consistent with political reality:

1. One is the problem of indirect influence, a phenomenon whose existence and importance are accepted both by the neo-elitists and by their opponents. For the reasons stated earlier, it is impossible under most political conditions to attribute power to people on the ground that their probable responses are anticipated (realistically or not) by political actors, particularly if such indirect influence is manifested in repeated anticipations and if it must be measured in terms that permit comparison with power exercised more actively and consciously.

2. How is power to be attributed to the lonely, frequently defeated *avant-garde* advocates who raise issues and dramatize proposals that eventually become law? Such mavericks often play an important role in focusing public attention, defining issues, and changing the terms of public debate.[84]

3. Bachrach and Baratz surely are right in arguing that institutions and procedures shape the outcome of decisions. Do the people responsible for the adoption of contemporary procedures have a sort of legatary claim to a share of the power involved in subsequent adoption of substantive measures that might not have passed without the procedural change? For example, do the successful sponsors of the 21-day rule in the 89th Congress deserve to be recognized as powerful on each of the bills that were passed under this rule?

The most useful field research concerning power—Dahl's and Edward C. Banfield's—has not made the claims to completeness demanded in the abstract writings on the subject.[85] In view of the problems involved in trying to live up to the theoretical literature, this modesty is both inevitable and desirable. I think it likely that future empirical work on political power will continue in this pattern, illuminating aspects of the subject but never approaching either "precise" or "complete" explanations. Indeed, much useful research relevant to one aspect or another of power does not even use the term or any of the associated vocabulary, and little seems to be lost for this omission.[86] In this complex and imperfect world, this does not seem to be deplorable, particularly if one thinks that the value of theory is measured by its utility in facilitating empirical research.

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[84] Cf. Ralph K. Huitt, "The Outsider in the Senate: An Alternative Role," *The American Political Science Review*, 55 (September, 1961), pp. 566–75.

[85] See Banfield's *Political Influence* (Glencoe, Ill.: The Free Press, 1961). For a discussion of the pitfalls in trying to describe a "power structure," see *Who Governs?* ch. 24.

[86] Many specialists on power are pessimistic about its utility for field research. See Riker, p. 348; and James D. March, "The Power of Power," in David Easton, ed., *Varieties of Political Theory* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1966), p. 70. Dahl has written that "Power has turned out to be a concept that seems highly useful so long as one does not demand that it be defined." See his "Power, Pluralism and Democracy: A Modest Proposal," paper delivered at the 1964 annual meeting of the American Political Science Association, Chicago, p. 5.

Comment: On Issues and Nonissues in the Study of Power*

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Introduction

This essay attempts to be at once a comment on some of the many significant points raised in Professor Wolfinger's article and a statement of a perspective on the issue of the "nonissue" in community power analysis.[1] It is *not,* however, intended as another salvo in the "elitist-pluralist controversy."

The dispute between "elitists," "pluralists," "neo-elitists," "neopluralists?" *et al.* has been much with us. A number of valuable ideas regarding approaches and methods for power analysis have, of course, emerged from the debate, especially in its earlier stages. With them, however, seems to have come a conspicuous friction which, I believe, increasingly impedes research. The main problem, as it strikes this noncombatant, is that each side has been reluctant to grant much to the other, while the language has been painfully polemical at times. To one who has learned from both camps and wants to advance the assault on persistent problems rather than on each other, the quarrel has become unfortunate. Debating points often obfuscate truly important issues for power analysis.

One illustration of the excessively tendentious character of the controversy seems to be the very labels attached to the parties. The term "elitist" seems inaccurate and invidious. It usually denotes a person who favors elite influence, something that is by no means true for the analysts tagged with this label. "Pluralist" has been more positively regarded in our culture up to now, but its overtones are also misleading in certain cases. My own feelings are well expressed by Dahl's response when he was categorized an "elitist:"[2]

> I, for one, object to being labelled "elitist" not only because . . . it would be inaccurate in implication even if it were a neutral term, but even more so because in our language and in our society it is unavoidably, I think, a pejorative, even a polemical epithet.
>
> . . . to stick the label "elitist" on someone is to discredit half his argument without saying another word.

One of the great virtues of Wolfinger's article, it seems to me, is that although it is forcefully presented, its spirit is that of contributing to a common effort to explore important problems. While I disagree with some of his judgments, this essay is presented in the same spirit, and a similar comment could be made concerning its relationship to the valuable work of Bachrach and Baratz.[3] Hence, at the beginning of these observations rather than at the end I should like to borrow a phrase used by Professor Wolfinger in another context and make "a plea for a decent burial" of the "elitist-pluralist controversy." It should be replaced by a diverse but joint assault on the important outstanding problems of community power analysis.

The Problem of Selecting Issues

In the first part of his article, Wolfinger focuses on one of the truly critical problems of political analysis—that of selecting the issues, policies, or decisions through which a political system is to be examined or several systems compared. In power analytic language, this is the problem of choosing the "scopes" (influenced behaviors) through which power structures and processes are to be inspected.[4] This

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* I should like to express my appreciation to Nelson Polsby for his helpful comments, his encouragement, and his patience.

[1] Raymond E. Wolfinger, "Nondecisions and the Study of Local Politics," *American Political Science Review,* 65 (December, 1971). The rest of my discussion assumes familiarity with Wolfinger's article. Further references to it will not be provided.

[2] Robert A. Dahl, "Further Reflections on 'The Elitist Theory of Democracy,'" *American Political Science Review,* 60 (June, 1966), 297–298, n. 7.

[3] Peter Bachrach and Morton S. Baratz, "Two Faces of Power," *American Political Science Review,* 56 (December, 1962), 947–952; "Decisions and Nondecisions: An Analytical Framework," *American Political Science Review,* 57 (September, 1963), 632–642; and *Power and Poverty: Theory and Practice* (New York: Oxford University Press, 1970).

[4] Minimally, a power relationship has five essential aspects: an *influencer,* an *influencee,* an *influential behavior* by the influencer, a *scope* (or response) by the influencee, and a *setting* in which the relationship occurs. Scope can be defined as the alteration in the state of the influencee produced, at least in part, by the influencer. The concept was first suggested by Harold D. Lasswell and Abraham Kaplan, *Power and Society* (London: Routledge & Kegan Paul, 1952), p. 77. Incidentally, the relationship between "scope" and "issue" or "issue-area" is frequently quite complicated. The latter two notions usually involve a complex pattern of scopes and influential behaviors.

Summary descriptions of much basic conceptualizaproblem pervades all political analysis, regardless of one's stance on the nondecision. The latter merely complicates matters, albeit greatly. It is a problem which must be handled by all techniques for locating power, including decisional analysis, attribution, opinion change, direct observation, attempts/successes, or any other method.[5]

Cut to the bone, the basic considerations seem to be the following: In all but the smallest and simplest social units, an extremely large variety of issues might be examined. Certainly this is true for any unit as complex as a town or city. And it remains true even if one capably tries to combine issues into broader "issue-areas," as was done in New Haven, for example. Thus, in addition to urban redevelopment, public education, and political nominations, one might study tax policies, transportation, welfare, recreation, health, employment, law enforcement, and so on, to say nothing of less traditional categories such as ecology, public morality, intergroup communications, or the aesthetic climate. Moreover, each of these rather gross issue-areas can itself be regarded as a conglomerate of many more specific issues, not all of which are usually investigated but which are instead themselves sampled.

Such a situation means that the researcher must select for his study a manageable subset from the very large universe of issues or policies which might be examined. As the "pluralists" and other careful analysts of power have long noted, however, findings about power may differ according to the issue selected. For instance, the power configurations for tax matters may diverge markedly from those for civil rights. If there is much variation of this type, a researcher could knowingly or unwittingly bias his results by choosing special, limited, or inappropriate issues, Hence, the scrupulous scholar confronts the formidable task of justifying his particular selection of issues through which power is analyzed. How is he to do this?

Current Approaches to Issue Selection. A number of researchers have pursued the convenient if unconvincing tactic of ignoring the problem. One such group only inquires about power in global, undifferentiated terms, without specifying any discrete issues or issue-areas. At best, they leave issue choice up to their respondents, though how these individuals resolve the problem no one knows. The deficiencies of such a tactic have been strongly scored in the literature, and nothing need be added here.[6]

A second group which ignores the problem has simply chosen to plunge ahead in pursuit of a small and arbitrarily determined set of issues, without attempting to justify selection. If the matter were left there, little damage would be done. In fact, such an approach can have a modest virtue, as I shall argue in a moment. But, alas, it has proved very difficult to resist the temptation to gain significance by generalizing from those few arbitrarily selected issues to "*the* power structure of the community," and even from that particular community to "the typical American community."

On the other hand, those analysts who have dutifully confronted the problem of justifying their selection of issues have usually discerned two basic strategies. They maintain either that the set of issues chosen was *representative* of the larger universe of issues which might have been chosen, or else they contend that they have fastened upon the most *important* issues.[7]

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tion for power analysis can be found in Robert A. Dahl, "Power," in David L. Sills, ed., *International Encyclopedia of the Social Sciences* (New York: Macmillan and The Free Press, 1968), Vol. 12, 405–415; Dorwin Cartwright, "Influence, Leadership, Control," in James G. March, ed., *Handbook of Organizations* (Chicago: Rand McNally, 1965), pp. 1–47; and Frederick W. Frey, "Developmental Aspects of Administration," in J. P. Leagens and C. P. Loomis, eds., *Behavioral Change in Agriculture* (Ithaca: Cornell University Press, 1971), Chapter 7; Frey, "Political Science, Education, and Development," in Joseph Fischer, ed., *The Social Sciences and the Comparative Study of Educational Systems* (Scranton: International Textbook, 1970), pp. 349–408.

[5] It follows that one criterion for evaluating techniques for locating and measuring power relations is consideration of how many and what kinds of scopes (activities, issues, decisions) they enable one to examine, at specified costs. Though it has many advantages, a disadvantage of reconstructive decisional analysis is that it tends to be so expensive in time and skilled labor that very few issues can be considered. For various reasons, the same tends to be true for direct observation, opinion change, and several other power measurement techniques. A more general and systematic comparison of the main devices for locating power is presented in Frederick W. Frey, "The Determination and Location of Elites: A Critical Analysis," paper presented at the 66th Annual Meeting of the American Political Science Association, Los Angeles, California, September, 1970.

[6] For example, see Raymond E. Wolfinger, "Reputation and Reality in the Study of 'Community Power,'" *American Sociological Review*, 25 (October, 1960), 636–644, and Herbert Kaufman and Victor Jones, "The Mystery of Power," *Public Administration Review*, 14 (Summer, 1954), 205–212. There have been several attempts to evaluate empirically the unspecified and specified scope approaches, *e.g.*, Martin Patchen, "Alternative Questionnaire Approaches to the Measurement of Influence in Organizations," *American Journal of Sociology*, 69 (July, 1963), 41–52, or Robert E. Agger, "Power Attributions in the Local Community: Theoretical and Research Considerations," *Social Forces*, 34 (May, 1956), 322–331.

[7] This categorization and, to the best of my knowledge, the first explicit discussion of the problem were

Since there are further variations on these two themes, let us briefly scrutinize them more closely.

Justification through Representativeness. The fundamental problem with selecting issues allegedly representative of all issues in the system has been well stated by Polsby: "There seem to be no satisfactory criteria which would identify a universe of all decisions [issues] in the community so that a sample of 'typical' or 'representative' decisions could in principle be drawn."[8] Ordinarily, the universe of actual (let alone submerged or potential) issues is so numerous, so varied, so subjectively defined, and so uncertain in its boundaries that it eludes sufficient specification to permit meaningful sampling. Put another way, there usually appears to be no acceptable sampling frame to allow the construction and evaluation of a representative sample.

What has just been said, however, needs qualification. Much depends on the meaning given to the term "issue" or "decision." Many studies have left these crucial notions undefined, and there are also certain problems with the explicit or implicit conceptions that have been used, as we shall see later. Nevertheless, some interpretations of "issue" and "decision" do ease the sampling problem noted above. For example, one can focus on the formally enunciated decisions of government, as manifested in bills, laws, regulations, hearings, and the like. Over any limited time period, this is likely to be a manageable, enumerable set whose sampling offers no great practical problems. Similarly, though slightly more broadly, one can define issues in terms of those topics that appear in community newspapers and other media over a stipulated period. Or one can rely on participant observers to list what they regard as "key issues" and decisions. If the conception of each issue or decision is quite global, so that fewer are needed to cover the set of activities involved, such listing may even be small enough to permit a complete inventory rather than a sample. Thus, in Oberlin, Ohio, Wildavsky and his associates contended that "the ease of working in a small town permitted us to choose every single decision (and most certainly every controversial one) of any importance from November 1957 to June 1961."[9]

Many will find such approaches to the sampling problem unsatisfactory on various grounds. For instance, the development of an effective description of the universe of "issues" or "decisions" (a sampling frame) is accomplished by such restriction of the basic meaning of those terms as to render them inappropriate. The focus becomes restricted to the formal activities of one agency—government. Though that agency and its activities are significant, many researchers define their analytic interests much more broadly. Local governmental decision making is only one aspect of community power for many scholars.

Also, the question always remains whether all issues were indeed covered by the described procedures. One must probe how newspapers and other media, or how participant observers, form their interpretations of what constitute issues. Unfortunately, researchers generally fail to provide adequate information to the reader for making these judgments. Unless an extremely narrow conception of what constitutes an "issue" or "decision" is taken, any convincing support for the issue judgments made by mass media or participant observers would again require delineating the relevant universe in order to show that it had been well sampled. For these and other reasons, it is not surprising that relatively few scholars have explicitly asserted that their studies dealt with a representative sample of all issues or decisions.

Justification through Importance. More common has been the effort to support the particular set of issues analyzed by contending that it includes important or key matters. Using this approach one can develop a more cogent general justification than through attempting to argue representativeness, but profound problems still remain. Right at the start we see, in my opinion, one of the less happy legacies of the "elitist-pluralist controversy." Very few analysts have asserted that they were able to examine *all* important issues. Rather, they contend that all the issues they have examined are important—i.e., the cases investigated include no trivial issues. Viewed baldly, such a strategy poses an immediately apparent problem. Because a selection includes no trivial issues, only important ones, does not mean that it is unbiased or generalizable, unless one makes the major assumption that patterns of power will be the same for the entire class of important issues.

What, then, is the appeal of such an approach? The answer is that it can be used to refute strong assertions of a dominant elite—assertions which charge that a ruling elite con-

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presented in Nelson Polsby, *Community Power and Political Theory* (New Haven: Yale University Press, 1963), pp. 95–97.

[8] Polsby, *Community Power* . . . , p. 96.

[9] Aaron Wildavsky, *Leadership in a Small Town* (Totowa: Bedminster Press, 1964), p. 8.

trols on all issues of importance in the community. Locating demonstrably important issues for which this is not true undermines such assertions. It may or may not be a very good strategy for determining what are the theoretically most instructive patterns of power in the community under scrutiny, but it is a useful strategy for answering popular though extreme "elitist" hypotheses in the literature. Once that thicket is cleared, however, the deeper problems with this approach emerge.

Probably the most basic difficulty is with the notion of importance itself. To employ this justification, one must specify the criteria by which importance is to be judged. Several suggestions have been made, a few quite sensible or ingenious. But up to now, none have been able to eliminate the basic element of subjectivity or relativism that plagues interpersonal comparisons of utility, welfare, interests, and importance.

Criteria proposed for establishing the importance of issues fall largely into two categories, one subjective and the other ostensibly objective. Under the former, an attempt is made to let the participants themselves determine the importance of issues, while under the latter the researcher usually determines it according to more commonly observable data.

The subjective approach is predicated on the idea that issues are important if the actors involved *feel* they are important. One might, for example, ask a sample of the community to designate for a given period all community issues they felt were very important, and then focus his power research on those mentioned beyond some arbitrary level of frequency. He might even obtain additional measures of the intensity of feeling associated with ascribed degrees of importance.

Such a procedure, which has not yet been widely used in power research, has severe limitations, however. These revolve mainly about the issue of weighting and aggregating the responses. How can interpersonal differences in the magnitude and intensity of feeling be determined and convincingly weighted? One man's "very important" is often not the same as another's. Does the fifty per cent who feel that an issue is "moderately important" equal the twenty five per cent who feel it is "very important"? Should actors be weighted not only in accordance with the degrees and directions of their feelings but also in accordance with their power positions in the community? In short, should one merely count heads and designate the issues with the greatest degrees of ascribed importance as the key issues? Or should one instead follow some more sophisticated weighting scheme—and, if so, which one and why? These long-standing questions of normative philosophy have not yet succumbed to any ready formula. It seems very unlikely that they will now surrender because a solution is needed for issue selection in community power analyses. Determining important issues through subjective ratings by participants also necessarily neglects some types of "nonissues" or "nondecisions" discussed in the following section.

Because of the comparability barrier confronting subjective definitions of importance, most plausible criteria offered by competent analysts fall into the ostensibly objective category. Nonetheless, the fundamental objection to them is that they, too, ultimately cannot escape the subjectivity which largely precludes comparison and demonstrably valid rankings. Either the researcher intrudes his own values, with which others may reasonably disagree, or else the supposedly objective criteria can be seen finally to depend on assumptions about subjective evaluations of the type just described.

The two most sophisticated and explicit attempts to grapple with the problem from this perspective have been made by Polsby and by McFarland.[10] Polsby suggests four criteria for ranking decisions according to their importance: 1) how many people are affected by the outcomes, 2) how many different kinds of community resources are distributed by the outcomes, 3) how much in amount of resources is distributed by the outcomes, and 4) how drastically present community resource distributions are altered by the outcomes.[11]

These are thoughtful suggestions, but they depend upon implicit value judgments which many might not share. Setting as a criterion how many people are affected by the outcome weights people equally, without regard for how intensely those people feel about matters or who they are. The number of kinds of different resources affected and the total amount of resources distributed (measured in what kinds of units?) both ignore qualitative distinctions. And using the criterion of how drastically the present community resource distributions are altered ignores trends and expectations, making the status quo the preferred standard for evaluating change. Moreover, what is meant by "drastic"? Who is to say if a fifty per cent alteration in one resource is as drastic as a thirty per cent alteration in another? More profound examination of these problems would reveal that subjective evaluations must ultimately re-enter the criteria, which leads to the possibility of logically irreconcilable disagreement regarding the ranking of issues in terms of their "importance."

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[10] Polsby, *Community Power . . .*, pp. 95–96; Andrew S. McFarland, *Power and Leadership in Pluralist Systems* (Stanford: Stanford University Press, 1969), Chapters 5 and 6, pp. 70–124, esp. Chapter 5. Also see Terry N. Clark, "The Concept of Power," in Terry N. Clark, ed., *Community Structure and Decision-Making: Comparative Analyses* (San Francisco: Chandler, 1968), pp. 69–71.

[11] Polsby, *Community Power . . .*, p. 96.

In an interesting recent discussion, McFarland accepts Polsby's four criteria of issue importance, but finds them incomplete and adds at least four others:

> Polsby's four criteria are observer oriented: they do not depend on the perceptions of the actors. At least four other criteria for selecting important issues, which do focus on the perceptions of the actors, are: (1) the number of people actually engaged in the political issue; (2) the amount of money, time, and other resources engaged in the issue; (3) other measures of the intensity of conflict, particularly violence or near violence; (4) whether or not the issue involves possible changes in the procedural rules of the game.[12]

McFarland labels these additions "activity criteria." They appear to contribute a useful dimension, but they also incorporate further implicit and contestable value judgments, while still probably not doing justice to the feelings of the participants. For example, how can one infer evaluations from amounts of money, time, etc., without subjective weightings of the value placed upon these resources? How legitimate is it to assume that for all groups the resort to violence indicates more intensely important issues than do other forms of political activity? In short, the *perceptions* and *evaluations* of the actors can by no means be automatically inferred from the *activities* described.

McFarland also offers another vehicle for distinguishing important issues. Building on earlier distinctions between "critical" and "routine" decisions, he equates importance with decisional generality in the context of a hierarchical tree structure of interrelated decisions.[13] Essentially, more important decisions are those that disproportionately determine or shape other decisions. An important issue has many more consequences for other issues than does an unimportant one. For a given decisional unit, individual or collective, it is often possible to order hierarchically nested sets of interrelated decisions, i.e., to perceive an integrated and coherent structure of choices. The most antecedent and determinative of these decisions, choices, or issues are regarded as most important.

Once again, this perspective, which he calls the "behavior-tree model," has much intuitive appeal. But it ultimately confronts the same problem of subjectivity that besets the other proposed criteria. McFarland candidly notes four serious "limitations" to the scheme he suggests.[14] One of these is that neither individuals nor organizations display the fully coherent and integrated structure of choices envisioned by the model. Numerous limited or partial structurings seem to be the rule, rather than a single, complete and fully integrated one. This fact, in turn, raises the profound problem of how to assess the relative importance of issues located in different hierarchies.

Second, the level of generality itself, and thus of importance, depends fundamentally upon the subjective values of the rater. To use McFarland's example, for one person taxation may be more important than education, so that educational choices flow from more basic choices regarding appropriate levels of taxation. But for another person, education may be more general and important than taxation, so that tax policy flows from more basic choices regarding educational objectives. Confronted with these divergent evaluations, how is one to arrive at a single assessment of importance? How can one aggregate ranking differences among actors? We are again faced with the same ineluctable problem of subjectivity and comparability previously encountered with regard to other proposed criteria.

I have mentioned these problems with the criteria for assessing issue importance suggested by Polsby and by McFarland because they are two of the most acceptable and judicious efforts at stating such criteria. The criteria suggested by Bachrach and Baratz and well criticized by Wolfinger, for example, seem more dubious.[15] The crucial point, however, is that any such formulation of issue importance is essentially a value judgment and ultimately subjective. "Importance" is basically a normative and evaluative term. In the context in which it has been applied, it calls for a moral judgment, as opposed to a professional value judgment regarding which strategy is most likely to lead to theoretical development. Everything considered, selecting issues on the basis of a completely defensible determination of their normative importance seems nearly as dubious as selecting them on the basis of their general representativeness.

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[12] McFarland, *Power and Leadership* . . . , p. 82.

[13] McFarland, Chapter 6, pp. 93–124.

[14] McFarland, pp. 112–123.

[15] They state, in "Two Faces of Power," p. 950, that ". . . any challenge to the predominant values or to the established 'rules of the game' would constitute an 'important' issue; all else, unimportant." In *Power and Poverty*, pp. 47–48, they state that, "A key issue . . . is one that involves a genuine challenge to the resources of power or authority of those who currently dominate the process by which policy outputs in the system are determined."

An Alternative Approach. What, then, is the analyst of power to do? Must he give up and slink back to more traditional, narrow, noncomparable and noncumulative studies? Or can he discern some promising way out of the difficulty?

Perhaps the first point worth mention is that not only power analyses but other types of political research confront the same problem. Studies of "public policy" usually cannot commence with an exhaustive inventory of all policies. Analyses of community "outputs" must start by examining a limited selection of those outputs and must defend that selection. And even if local politics is conceived as municipal-government-plus-auxiliary-agencies, one cannot pretend to have examined all aspects of those institutions. Selection of foci and the justification of that selection are difficult problems for all political analysis, not just for studies of community power. Indeed, it might be construed as a virtue of the latter (and perhaps of the "elitist-pluralist controversy") that it has brought this recondite problem so clearly to the surface.

At this juncture it is instructive to reexamine the basic difficulty. Empirical political analysis must have a less than global focus. In discovering through specific empirical research how policies are made, how outputs are allocated, how power is exercised, or even how government works in some social unit, one cannot cover every aspect of an enormously multidimensional reality. Selection is essential.

If all studies involve focal selections, what then is the problem? Some readers might be uninterested in the selections made—the foci chosen—but that engenders no great intellectual or research problem as long as others are interested. As McFarland observes, however, ". . . the catch-all critique of an empirical decision-making analysis of political power is the assertion: 'You have studied the wrong issues.' "[16]

The phraseology here is critical. The issues studied are not branded uninteresting, pointless, or inconsequential, but "wrong." Wrong in what sense? Wrong in the sense that they do not accurately reveal the power structure of the community under study. They are unrepresentative. Wrong in the sense that generalization from them to the prevailing patterns of power in the social unit is misleading. Wrong in the sense that they suggest there is no elite when one really exists, or that there is an elite when one does not really exist. Wrong in the sense that from them the typical power structure is incorrectly inferred to be concentrated or dispersed when it is actually otherwise. Thus, for example, apart from considerations related to "nonissues," only a few have quarreled with Dahl's findings regarding power in New Haven for the three issue-areas studied.[17] Rather, they have usually suggested that two of the three (nominations and education) may be relatively unimportant in New Haven (at least to a putative elite), or else they have lamented that Dahl did not cover other issues, such as welfare or taxation, which would presumably have altered the picture of power in New Haven.

Much of the difficulty, therefore, seems to lie in the implicit goal that induces one to claim general representativeness or normative importance for the issues studied rather than presenting them in terms of whatever their intrinsic interest may be, arguing neither global representativeness nor undeniable importance. This implicit—and infectious—goal is generalizing to "*the* power structure" of the unit. That particular phrase may be eschewed, but it is replaced by expressions like "power in Sheboygan" or "the political system of Cheyenne," which, under scrutiny, turn out to mean the same thing. Even if the author tries to be careful to restrict his observations to power only in the issue-areas investigated, he is often careful only part of the time, lapsing into broader generalizations in his introductions and conclusions, while his readers frequently show no restraint at all. The result has been that the literature on community power has featured virulent debate about many topics that are clearly premature and sometimes probably unanswerable.

An alternative approach, not without lingering problems but significantly more promising, would be the following: We must recognize that the notion of "*the* power structure" of a social unit is a dangerously misleading siren. There are as many power structures as there are issues fruitfully distinguished. Some of these structures, perhaps most of them, may in certain kinds of units strongly resemble one another, though in complex units at least minor differences will always be found. One of the outstanding tasks for power analysis is to discover better ways of measuring and expressing these relationships—measures of the degrees of structural isomorphism and positional stability across the various issue-based power structures of a unit.[18] Such research should lead to intriguing theory about the kinds of units in which there is a high degree of isomorphism and positional stability and the kinds in which there is little, along with the consequences of these tendencies for other values. Harold Lasswell started this quest years ago with his conception of "agglutination."[19] Further thought and effort are now needed to move ahead.

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[16] McFarland, p. 87.

[17] Robert A. Dahl, *Who Governs?* (New Haven: Yale University Press, 1961). The areas were political nominations, public education, and urban redevelopment.

Since generalization to "*the* power structure" of the unit is presently pointless, power analyses must state their generalizing hypotheses much more carefully than they have heretofore. "Power in Oshkosh" is, almost by definition, an insufficiently specific notion. Instead we must always indicate the issue or scope involved. Communities or other social units can then be compared only within this framework. One might, for example, study the distribution of power with regard to education in five cities, not claiming to contrast their patterns of power in any respect other than that. Of course, as competent studies of the same communities were gradually done for other issues, one could examine the cross-issue structural similarities previously mentioned, assuming the divergent time periods did not preclude comparison. But meantime, until many issues have been systematically researched, the temptation to speak globally about "power in Subject City" must be resisted.

Such a strategy does not greatly diminish the significance of power research. It would be a fascinating contribution to understand even dimly the community variables most predictive of highly concentrated power patterns for decision making in public education and the variables most predictive of great dispersion of power in that area. The same applies to taxation, welfare, civil rights, and other issues. Overgeneralization is not needed to make such research relevant and significant; it can only diminish it. Nor should we mask the unavoidable subjectivity which enters the selection of basic areas for inquiry.

At least two remaining and related problems, however, are bared by the preceding analysis. The first is that even the great reduction in attempted generality here proposed does not in principle completely eliminate all the difficulties previously discussed. Issue-areas are composed of numerous issues. Issues may be composed of numerous decisions. More general decisions may be composed of less general ones. At any chosen level, often only a part of the subset of relevant cases can be examined. In fact, general analytic categorization of a continuing, concrete reality implies this. Hence, even at markedly lower levels of generalization, selection and inference are still present.

The crucial difference, however, is that at lower levels of generalization, effective conceptualization of the relevant universe of subissues or decisions becomes much more feasible, and the possibility of representativeness in selection more real. Part of the art of power analysis is to find the maximal level of generality at which this effective conceptualization is possible. That maximum is patently less than the universe of all issues for the total community, for instance. It may even be well below the level of such a broad issue-area as public education or urban redevelopment. But the trick is to find the most general level at which one can validly operate. Anyone who has done much empirical research on power probably gives great weight to his impressions of how effectively the participant respondents seem to conceptualize at various proposed levels of generalization. Does the suggested conception of the issue or decision seem to have real impact for essential respondents or observers, or does it fail to engage them? Have at least some seemingly astute observers developed plausible confidence in their ability to describe all the major dimensions, aspects and subgroupings of the issue? Though less than perfect, these are vital practical guidelines to locating an effective level of issue or scope generality. Even firmer and more objective guidelines are needed. When they are found and used, researchers much restrict their generalizations to the areas investigated.

The other prominent problem highlighted by this analysis is common to all comparative research, even though it has not been much discussed in connection with issue selection for power studies. It is the problem of *equivalence.* One may, for instance, investigate the issue-area of public education in two communities. Ordinarily, in each community the specific issues which comprise this issue-area will overlap to some degree and diverge to some degree. In one community the hiring of a new superintendent may be a salient decision. In the other that decision may not be present at all, but the acquisition by the city of a parochial school building may be prominent. Several other similar discrepancies in the specific issue indicators which make up the issue-area may also exist. Hence, the obvious query arises: are the two issue-areas of "public education" truly equivalent in these communities? What does "equivalence" mean, conceptually and operationally, in this context? How can it be demonstrated?

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[18] An introduction to these concepts is furnished in sections of the articles referred to in note 4.

[19] Lasswell and Kaplan, pp. 57–58. "Agglutination" refers to a situation in which the structural ". . . positions of a person or group in different value patterns [power, wealth, prestige, etc.] tend to approximate one another." ". . . The patterns of different values tend to coincide."

It is not possible in this discussion to explore the matter more fully. Available treatments of equivalence in comparative research seem quite appropriate.[20] However, a specific means of handling the problem will have to be developed for comparative research on patterns of power in social units.

The "Nonissue" as a Research Problem

The prime concern of Wolfinger's article is the "nonissue" or "nondecision," most recently brought to our attention by Bachrach and Baratz and by Anton.[21] Wolfinger raises a number of probing queries about the fruitfulness and feasibility of this concept for empirical political analysis, presenting at least four major contentions. The first and most important is that the very conception of a "nonissue" or "nondecision" is inadequate. The second is that many critical aspects of nonissues cannot practically be researched. The third is that the data which might be gathered on nonissues do not provide an adequate basis for conclusions about the distribution of power. And the fourth is that the concept of power structures, and perhaps even of power itself, poses insurmountable difficulties.

Though appreciating many insightful points included in these arguments, I would disagree with each of the major contentions. Rather than take them up *seriatim,* I shall attempt to present an alternative approach to the same topics. The discussion will focus first on the notions of what is an issue and a nonissue, second on the researchability of nonissues, and third—very briefly—on power structures and power analyses, including the kinds of conclusions which might be drawn from investigations of nonissues.

Identifying Issues and Nonissues. At first blush, a nonissue or nondecision may seem to be merely the complement of whatever is defined as an issue or decision. Actually, this is not the case, suggesting yet another instance of the poor labeling and notation nearly endemic among social scientists. "Suppressed issue" might be a more accurate expression.

Be that as it may, it is nonetheless useful to begin with a glance at the basic conceptions of an issue and a decision. Both terms, but especially "issue," are seldom defined in the community power literature. A dictionary definition of an issue generally refers to "a point, matter, or question to be disputed or decided." One thinks immediately of phrases such as "at issue," "join issue," or "take issue." As McFarland remarks, ". . . common connotations of 'issue' include some amount of controversy or confusion."[22]

Of the two concepts, "decision" is usually the more basic. It also is more frequently defined. Almost all definitions interpret decision as a choice among alternatives. An issue can then be correspondingly defined as a matter calling for or involving a decision. Another related term is "issue-area," as used in the New Haven study. To the best of my knowledge, Dahl, like most other writers, fails to define issue or issue-area in *Who Governs?* The implication, however, seems to be that an issue-area consists of an interrelated set of issues (he calls them "decisions"). In *Who Governs?,* for example, the issue-areas of public education and urban redevelopment each included eight issues arising during the decade of the 1950's.[23] In general, then, we have three related concepts in common use: decision, issue, and issue-area.

Issues and Conflict. The central connotations of the term "issue" suggest contest and controversy. Although it is neither logically nor definitionally necessary, the assumption that power implies conflict and is only revealed by it is made in many power analyses oriented toward issues and decisions. Such an assumption is both venerable and understandable, although unfortunate in my opinion. Dahl, for example, apparently employed it in his seminal article, "A Critique of the Ruling Elite Model;" there he contended that the hypothesis of a ruling class can be strictly tested only if there are ". . . cases involving key political decisions in which the preferences of the hypothetical ruling elite run counter to those of any other likely group that might be suggested," and ". . . in such cases, the preferences of the elite regularly prevail."[24] The popular Weberian notion of power as realization of one's will despite the resistance of others was an earlier and still more influential impetus in the same direction.[25]

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[20] I have attempted to deal with equivalence in "Cross-Cultural Survey Research in Political Science," in Robert T. Holt and John E. Turner, eds., *The Methodology of Comparative Research* (New York: The Free Press, 1970), pp. 173–294, esp. pp. 187–190, 232–233, 240-257, and 284–288.

[21] Bachrach and Baratz, "Two Faces of Power"; Thomas J. Anton, "Power, Pluralism, and Local Politics," *Administrative Science Quarterly,* 7 (March, 1963), 425–457.

[22] McFarland, p. 122.

[23] Dahl, p. 333.

[24] Robert A. Dahl, "A Critique of the Ruling Elite Model," *American Political Science Review,* 52 (June, 1958), p. 466. I find it somewhat ironic that Bachrach and Baratz are among the most emphatic in building conflict into the very conception of power itself: ". . . In order for a power relation to exist there must be

The assumption that power implies conflict arises from several very legitimate concerns shared by most serious students of power. Outstanding among these are recognition of the need to distinguish *influenced* behavior from *autonomous*, self-directed behavior; the need to study power through empirically verifiable procedures and to eliminate subjective intrusions as far as possible; and the related need to avoid what Dahl aptly characterized as an "infinite regress of explanations."[26] For successful research, the concept of power must be adequately operationalized and studied with replicable procedures. This, in turn, means that overt behavioral evidence is ultimately essential to support assertions about the nature and distribution of power.

These principles, strongly insisted upon by the New Haven group and others, seem unexceptionable. They do not imply, however, that conflict, under any ordinary definition of that term, is necessary for power to be manifested. Indeed, the very conception of power itself, as well as recollection of some of its real and imagined manifestations, indicates the error of assuming that power necessarily implies conflict.

In one of the most influential formulations, Dahl roughly defined power as one actor's ability to get another to do something he would not otherwise have done.[27] Wolfinger perceptively points out that ability really defines *potential* rather than actual power and may be somewhat at variance with the notion of power implicit in some of Dahl's other writings. But the obvious emendation is easy to make. Let us define power as a relationship such that the behavior of one actor (individual or collective) alters the behavior of another actor.[28] But neither under Dahl's potential power conception nor under this actual-exercise conception is there any stipulation that the person influenced (influencee) does not want to do what he is influenced to do. The deliberate stipulation is merely that the person influenced would not have acted as he did were it not for the action of the influencer in the given situation. The behavior of the influencer must be adduced to explain the behavior of the influencee.

There are numerous important instances in which power so defined is clearly exerted but in which conflict (regarded as visible contention, controversy, or even as opposition of interests) is absent. Among these are the cases in which: 1) the influencer induces the influencee to pursue one rather than another of several attractive and mutually exclusive alternatives; 2) the influencer's power is the result of the influencee's identification with the influencer, and his concomitant desire to be and to appear to be influenced by him; 3) the influencer has socialized the originally indifferent influencee to want to behave as the influencer wishes him to behave.

A fanciful example—one hopes—which illustrates tremendous power exercised without conflict might be Huxley's *Brave New World* or, better yet, Skinner's *Walden Two*.[29] In each case, covert conditioning methods are used to shape individuals to fit societies they never made. Conflict, at least from internal sources, is presumably absent, the clash of interests totally silent; yet enormous power is clearly being exercised.

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a conflict of interests or values between two or more persons or groups." ("Decisions and Nondecisions . . . ." p. 633). They, however, define "power" to be what I should call a subtype of power, along with authority, manipulation, force, etc., and they have no generic term for what is common to all these relations. At least two of their power concepts, nonetheless, permit power without conflict—viz., authority and manipulation.

[25] Max Weber, *The Theory of Social and Economic Organization*, edited by Talcott Parsons (London: The Free Press of Glencoe, 1964), p. 152: "'Power' (*Macht*) is the probability that one actor within a social relationship will be in a position to carry out his own will despite resistance, regardless of the bases on which this probability rests." Or, cf., "In general, we understand by 'power' the chance of a man or of a number of men to realize their own will in a communal action even against the resistance of others who are participating in the action." H. H. Gerth and C. Wright Mills, eds., *From Max Weber: Essays in Sociology* (London: Kegan Paul, Trench, Trubner & Co., 1947), p. 180. Mills, for example, adopted the Weberian conception in *The Power Elite* (New York: Oxford University Press, 1956), p. 9, as Wolfinger notes. According to my interpretation, however, the Weberian conception and Dahl's, though perhaps alike in referring to a potentiality rather than an actuality, differ critically in that Weber builds the intention of the influencer into the definition whereas Dahl does not. In the first formulation, Weber may even be taken to build resistance, as commonly rather than power analytically defined, into the conception of power. Dahl's more general formulation seems much preferable. The distinctions regarding intention and resistance can then be included via subordinate conceptualization.

[26] Dahl, "A Critique . . . ," p. 464.

[27] Robert A. Dahl, "The Concept of Power," *Behavioral Science*, 2 (July, 1957), 201–215.

[28] Frey, "Political Science, Education, and Development," pp. 365–369. Extremely useful explications of the concept of power are James G. March, "The Power of Power," in David Easton, ed., *Varieties of Political Theory* (Englewood Cliffs, N.J.: Prentice-Hall, 1966), pp. 39–70, and William Riker, "Some Ambiguities in the Notion of Power," *American Political Science Review*, 58 (June, 1964), 341–349, though in each case, in contrast to Wolfinger, I should argue that the pessimistic conclusion is not justified by the discussion.

[29] Aldous Huxley, *Brave New World* (London: The Vanguard Library, 1952); B. F. Skinner, *Walden Two* (New York: Macmillan Paperbacks, 1962).

Examples from "real life" in which elites appear to shape the political culture so strongly that no conflict occurs, even though one might otherwise expect it, are not difficult to find. Dahl has pointed to some totalitarian systems as possible exemplars of overt conflict completely suppressed by overwhelming power.[30] Toward the other end of the spectrum, and less obvious than raw totalitarianism, at least some small towns in the United States seem *prima facie* to display more subtle elite domination without conflict. The "company town" of Kannapolis, N.C., if accurately portrayed by Ralph Nader's television documentary, is a possible case in point. Vidich and Bensman's "Springdale" is a less extreme illustration.[31] And we can certainly find many more examples of ostensibly conflict-free but profoundly controlled systems at the organizational and small group levels.

In fact, one of the best pictures of how the political culture of the community shapes and channels power ("mobilizes bias"), aiding some actors and impeding others, is provided not by Schattschneider, but by *Who Governs?*[32] For example, in earlier days, when the "patrician" elite ran New Haven, the system described was almost a model of a heavily controlled but conflict-free type. "The elite seems to have possessed that most indispensable of all characteristics in a dominant group—the sense, shared not only by themselves but by the populace, that their claim to govern was legitimate."[33] We need but entertain the plausible hypothesis that the popular belief in the legitimacy of the elite's power was to a large extent inculcated by elite-controlled institutions and we have a near classic picture of the relatively conflict-free, nondecisional system.

In most general terms, a nonmonotonic relationship seems to exist between the distribution of power in a social unit and the appearance of visible conflict. After a very high threshold of power concentration is reached, the probability of manifest conflict revealed in public decisions is markedly reduced. Hence, the analyst cannot assume that power must be manifested by visible conflict over issues. If it exists, conflict certainly affords one of our best insights into many aspects of the power situation. But it has clear limitations as a vehicle for examining several important power processes. Any research strategy, however useful it may otherwise be, which focuses solely upon conflicts and ignores other glimpses into the phenomena of power must be regarded as incomplete. Issue analysis does not compel such a limited focus, but it often seems to foster it.

A broader approach to empirical power research might be called activity analysis. It provides an orientation which comprehends issue analysis plus other topics that are often neglected because of the conflictual overtones surrounding the concept "issue." The specificity of focus demanded by empirical research is furnished by designating activities of interest rather than issues of interest. These activities, such as joining a political organization, paying taxes, employing workers, bribing, or rioting, are regarded as the possible scopes (influenced behaviors) of significant power relations. One attempts to discover which actors in the system, if any, influence which other actors with regard to the selected activities. The term "activity" may actually be too narrow, since demonstrable attitudinal change is also included.

Of course, the activities initially designated ordinarily lead to examination of other activities which produce them, leading in turn to still other activities, etc., in a chain of influence that is terminated either by the limitations of the data or arbitrarily by the researcher.

Usually, an issue or decision in a power analysis can be construed as a set of influenced activities or power relations. It is not merely a choice among alternatives, but additionally it is an *influenced* choice among alternatives or else a choice among alternatives that *influences* the activities of others. From a power perspective, creating the Redevelopment Agency and determining the educational budget are issues which can be expressed as more or less complex sets of influenced or influential activities. An activity focus performs the same heuristic and organizational functions as an issue focus; it is more complicated, but it is less likely to become restricted to conflictual phenomena. In most cases, it seems wise to include if possible both issue analyses and activity analyses in a power research design, though the activity analysis is difficult in units larger than small organizations of a few hundred people.

The focus of power studies utilizing either an issue analysis or a decisional analysis approach has often been further limited by the particular construction placed upon "community power." Once again, while power is often defined, community power is quite commonly left vague. Unfortunately, several divergent interpretations have been given to community power. The most general minimally involves designating the community of reference and the scope of the investigation (activity, issue, or decision), which may be any well defined focus of interest regardless of how many or few actors in the community are involved and whether they include its formal authorities or not. One can thus refer to power relations in Boston concerning professional football, power processes in Berkeley regarding the use of city parks, or power patterns in Belchertown related to fair employment practices. The scopes most interesting for political analysis, however, tend to be those involving power over the allocation and distribution of power itself. In every instance, though, the initial focus is presumed to include all potentially participant actors in the community, and government is regarded as merely one actor (or set of actors) among others.

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[30] Dahl, "A Critique . . . ," p. 468.

[31] Arthur J. Vidich and Joseph Bensman, *Small Town in Mass Society* (Garden City, N.J.: Doubleday Anchor Books, 1960).

[32] E. E. Schattschneider, *The Semi-Sovereign People* (New York: Holt, Rinehart and Winston, 1960).

[33] *Who Governs?* p. 17.

On the other hand, a great many studies explicitly or implicitly restrict community power to decision making in the name of the community, or decision making which affects all or at least a large segment of the community's population. Many studies go even further to confine community power to formal public policy making, customarily defined local "politics," or "municipal government," as Wolfinger overmodestly allows. One by no means denigrates these efforts by asserting that for many scholars community power includes much more than local governmental decision making having a wide impact.

It is sometimes claimed and frequently implied that almost all important community issues will at some point be funneled through local government, and so will be picked up by a focus on governmental decision making.[34] This seems to me a dubious contention. Our political culture, for example, apparently renders unto business certain important types of decision making that various other societies handle through government—many production, investment, location, and employment decisions, for instance. Although aspects of these decisions may sometimes involve local government, many crucial elements ordinarily do not. And while "business" cannot be assumed to be the dominant actor in community power structures, neither can local government. A focus primarily oriented to the latter may produce fascinating and valuable findings; but for many analysts it cannot purport to cover all facets of what they mean by community power.

Finally, we should note that many inconspicuous decisions and activities may cumulate into major patterns of power. As Janowitz observed about Banfield, many studies have been ". . . interested exclusively in controversies that generate big, dramatic decisions, but in the city . . . the legal, administrative, and political structure makes hundreds of decisions daily, out of which emerge crucial patterns of social change."[35] Everyday activities, such as certain reactions toward women or thousands of minor policies in the granting of financial credit, may almost unconsciously pervade the life of a community and be just as critical as any "momentous" controversy over urban redevelopment or welfare policy. In short, we must be wary lest a narrow interpretation of "community power" combine with a very conflict-oriented conception of an "issue" to produce a literature on community power structures that is empirically somewhat sounder, but misleadingly narrow.

The "Nonissue" and "Nondecision." As Wolfinger notes, Bachrach and Baratz, perhaps reluctant to abandon a convenient tradition, did not very explicitly define "nondecision" in their article, "The Two Faces of Power" (rather strange, since they implicitly taxed Dahl for not defining 'key political decision"). To be fair, however, they did generally indicate the meaning of "nondecision-making," viewing it as a process by which ". . . *status quo* oriented persons and groups influence those community values and those political institutions . . . which tend to limit the scope of actual decision-making to 'safe' issues."[36] In their second article, "Decisions and Nondecisions: An Analytical Framework," they describe "nondecision-making" as ". . . the practice of limiting the scope of actual decision-making to 'safe' issues by manipulating the dominant community values, myths, and political institutions and procedures." Later, in the same piece, they explain that, "when the dominant values, the accepted rules of the game, the existing power relations among groups, and the instruments of force, singly or in combination, effectively prevent certain grievances from developing into full-fledged issues which call for decisions, it can be said that a non-decision-making situation exists." It is different from merely deciding not to act or decide in a normal decision-making situation because in the case of nondecisions, the latent issue is *prevented* from ever becoming a question for decision.[37]

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[34] Though he is well aware of the limitations of the procedure, this is done, e.g., in Bradbury Seasholes, "Patterns of Influence in Metropolitan Boston: A Proposal for Field Research," in Bert E. Swanson, ed., *Current Trends in Community Studies* (Kansas City: Community Studies, Inc., 1962), p. 61.

[35] Morris Janowitz, "Community Power and 'Policy Science' Research," *Public Opinion Quarterly*, 26 (Fall, 1962), p. 405.

[36] P. 452.

[37] P. 641. In *Power and Poverty*, they provide two apparently similar definitions which, however, are in one sense significantly different. The first (p. 44) de-

Intuitively, it seems that Bachrach and Baratz spotlight an absolutely vital area for investigation. Out of the tradition of Friedrich's notion of "anticipated reactions" and Schattschneider's "mobilization of bias," they again call our attention to the tacit, covert aspects of politics.[38] How important these phenomena are in any specific setting is always an empirical question. Nevertheless, two initial impressionistic judgments seem valid: 1) There are at least some settings of significance in which such implicit or covert aspects of politics are crucial; and 2) these latent aspects of politics are sufficiently pervasive that a political science which ultimately cannot cope with them is likely to be an arid enterprise indeed.

It is not surprising, therefore, that very few scholars deny the potential significance of nonissues, even though their researchability is doubted and the conception of the nonissue is found unclear and perhaps excessively subjective. An illustration of this situation, and one of the numerous ironies in the "elitist-pluralist controversy," seems to me the fact that *Who Governs?*, the fulcrum of the debate, is replete with trenchant insights into the role of nonissues, political myths, dominant values, belief systems, preferential rules of the game, and cultural predilections as factors of extreme importance in New Haven politics. The discussion may not have taken the last step in flagging the nonissue nor have probed every cranny, but a constant concern with many aspects of the problem is clearly evident. It is difficult to see empirical political science proceeding very far without an adequate capacity for researching nonissues.

Despite the seeming importance of the topic broached by Bachrach and Baratz, one may feel less content with their particular formulation of the idea of the nonissue. For example, they imply that the nonissue is a tool for the preservation of the status quo by vested interests. Such may usually be the case, but it is not necessarily so and should not be implied by the very definition of the nonissue. Wolfinger accurately notes the utility that nonissue considerations can have in enabling masses to influence elites. One can also conceive of situations so clearly favoring change that the mobilization of bias is against the status quo (unless one wishes to define status quo in this case as the inertia toward continued change). Similarly, a nonissue situation can exist without any clear "grievance" being present. In short, it seems possible to refine the concept of the nonissue beyond the valuable start provided by Bachrach and Baratz.

A nondecision occurs when a choice among alternatives by one actor is either not perceived by him or, if perceived, is not made, and always, in either case, because of some exercise of power by another actor. A nonissue is a matter presumably calling for a decision but which is not perceived as such or, if perceived, is suppressed, always because of some actor's use of power. These ideas can be further generalized in a more rigorously power analytic framework. *The nonissue involves the effective use of power by some actors in a political system to deter other actors in that system from even attempting to exert influence.* Thus, anything not occurring or not chosen—any alternative foregone—is not a nondecision. Acceptance of one policy when others are not even discussed does not indicate a nonissue. The nonoccurrence, nondecision, or nonissue must be the result of the exercise of power by some actors in the system.

From this perspective, then, the two critical elements of the nonissue are *expectation* and *prevention*. The analyst must first justify his expectation of attempted influence by some actor—that is, he must show good cause for us to suspect that the lack of attempted influence by some actor, or the lack of a certain issue, is not merely due to an autonomous disinterest or decision. We must have good reason to anticipate attempted influence by such an actor under the given conditions, or to anticipate the presence of such an issue in the given system and setting.

Second, once the expectation of attempted influence or issue formation is established, the analyst must proceed to demonstrate the mechanism of prevention. This mechanism must be some form of the exercise of power—conscious or unconscious—by other actors in the system. One may speak loosely of rules of the game, dominant values, biasing beliefs, and the like; but these must be disseminated through some specifiable influence process if one is to argue

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fines a nondecision as ". . . a decision that results in suppression or thwarting of a latent or manifest challenge to the values or interests of the decision-maker," and the second (p. 57) says that a nondecision is "an attempt to prevent an issue from reaching the decision-making state." The last definition seemingly focuses on attempts rather than successful prevention and in that way is anomalous compared to their other formulations. But the major change in these conceptions—a significant improvement, I believe—is no longer to define the nondecision from the perspective of the status quo and an assumed dominant group. Since most of their work has not been in this vein, however, I have concentrated on their earlier formulations, which are essentially reiterated as the first two and one half chapters of *Power and Poverty*.

[38] Carl J. Friedrich, *Man and His Government* (New York: McGraw-Hill, 1963), chap. 11, pp. 199–215.

convincingly for a nonissue or suppression of influence attempts. Independently formed judgments of the rules of the game—beliefs not demonstrably inculcated by other actors in the system—hardly betoken a nonissue. Though decisional analysis may not always be the most appropriate research method, Dahl was quite right in insisting that there must be some point in the process of creating myths, false consciousness, biased beliefs, etc., at which the power process occurs. The nature of this process must be established for a nonissue argument to be convincing.

Such an analysis of the nonissue suggests that it has a mirror-image on the positive side which should be interesting. If the nonissue involves the use of power by some actors to *deter* others from attempting influence, its mirror-image refers to the use of power by some actors to *encourage* others to attempt influence. The mobilization of bias, for example, works both positively and negatively, deterring some actors from attempting influence or raising issues while inducing others to do those very things. The positive side, prompting some issues, may be no less important than the negative, suppressing others.

Types of Nonissues. Correctly regarding the notion of the nondecision as the principal contribution of Bachrach and Baratz, Wolfinger discerns three types of nondecisions. In an earlier article covering some of the same terrain, Merelman also saw three main varieties of nondecisions, and two of the three (types 1 and 3 below) seem to coincide with Wolfinger's categories.[39]

Perceived Types of Nondecisions

Wolfinger	*Merelman*
1. "Renunciation"—an actor who is already a participant in decision making refrains from some specific influence attempt because of anticipated negative reactions from others.	1. "Power argument"—nonelites anticipate negative reactions from elites and so do not even press their conflicts with them.
2. "Abstention"—an actor who is not already a participant consciously decides not to attempt a given influence, i.e., to withdraw and not participate, probably because of anticipated negative reactions.	2. "Force argument"—elites use coercive devices to forestall conflicts from ever reaching the public, governmental stage of decision making.
3. "Nonparticipation"—an actor who is presumably not already a participant and who is unaware of his interests therefore does not make any attempt to exert influence.	3. "False consensus"—cases where nonelites are encased in values foisted on them by elites and are therefore not even conscious of real conflicts with elites.

If this summary exposition of the types of nondecisions suggested by Wolfinger and Merelman is accurate, it seems that they are fairly similar and that both are incomplete, *prima facie*. The number of dimensions distinguished yields more types, even minimally, than are presented.

Wolfinger, explicitly or implicitly, fastens on at least three dimensions of the nondecision: 1) the *degree of restriction* on potential influence attempts (i.e., whether complete withdrawal from all attempts is involved, or merely abandonment of a much more limited subset of influence attempts); 2) the *degree of awareness* by the nondecider concerning his interests (i.e., whether he is consciously or unconsciously restricting his potential influence attempts); and 3) the *preventive mechanism* inducing the restriction on influence attempts (i.e., whether this is anticipated negative reactions or some other form of power).

Merelman also employs the awareness dimension and that of anticipated negative reactions (preventive mechanism). It is not altogether clear whether he implies any distinction regarding the degree of restriction on the potential influence attempt (withdrawal vs. more limited restriction), though he probably does not. On the other hand, he introduces additional dimensions with various ramifications into his "force argument." In this case, both the stage of elite intervention (pregovernmental) and the type of influential behavior the elite then employs (coercive) are specified.

Even if we rather crudely dichotomize the three dimensions of the Wolfinger typology (the degree of restriction, the degree of awareness, and the preventive mechanism), we are logically left with an eightfold classification. Some of the cells of this typology will probably be empty, namely the two cells involving the intersection of anticipated reactions and unawareness of interests. But the remaining six cells all seem meaningful:

1. The actor is aware of his interests, partially restricts his influence attempts, and is led to do this because of anticipated negative reactions. (Probably Wolfinger's "renunciation.")
2. The actor is aware of his interests, completely withdraws from influence attempts in

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[39] Richard M. Merelman, "On the Neo-Elitist Critique of Community Power," *American Political Science Review*, 62 (June, 1968), 451–460.

the given area, and is led to do this because of anticipated negative reactions. (Probably Wolfinger's "abstention.")

3. The actor is aware of his interests, partially restricts his influence attempts, and is led to do this for reasons other than anticipated negative reactions, such as confusion about how, when, and where to exert influence. (A good example is the "Springdale" situation referred to by Wolfinger.)

4. The actor is aware of his interests, completely withdraws from influence attempts in the given area, and is led to do this for reasons other than anticipated negative reactions.

5. The actor is unaware of his interests and thus partially limits his influence attempts. When the actor is unaware of his interests the preventive mechanism would seemingly have to be something other than anticipated negative reactions, e.g., inculcated false values, educational deprivations, etc.

6. The actor is unaware of his interests and thus completely neglects influence attempts in the given area. (Probably Wolfinger's "nonparticipation.")

While this elaboration is far from earth-shaking, types three and four do seem significant. Their absence from the other typologies seems more generally indicative of an insufficient amplification of the mechanisms available to induce nondecisions. Such mechanisms are much more diverse than the "anticipated reactions," "foisting of values" or "false consensus" arguments seem to suggest. They lead into as yet largely unexplored areas of political socialization. Especially important are inculcated conceptions of legitimacy, learned preferences among influential behaviors, expectations of failure, and confused perceptions of power processes and structures. Such orientations are less clear perhaps than anticipated negative reactions, but possibly no less effective as deterrents of influence attempts.

The Researchability of Nonissues. In concluding "Two Faces of Power," Bachrach and Baratz ". . . reject in advance as unimpressive the possible criticism that this approach to the study of power is likely to prove fruitless because it goes beyond an investigation of what is objectively measurable."[40] Though Bachrach and Baratz may be unimpressed by this difficulty, Wolfinger and others obviously find it impressive, even intimidating. The gravamen of the case against the nonissue, in fact, is that it cannot be empirically researched without the intrusion of subjective ideological assumptions.

An appropriate general stance on this disagreement seems clear. Plainly, one must operate somewhere between a metaphysical disdain for measurement and a paralytically fastidious operationalism. The potential importance of the matters referred to under the rubric of the nonissue is a strong factor in its favor. As many have said, better a poor measurement of an extremely important phenomenon than an excellent measurement of a trivial one. Yet there are clear limits to this doctrine. To be useful in empirical science, notions such as the nonissue must have reasonably clear, adequately objective, and accessible empirical referents.

Since the concept of the nonissue is relatively recent and so important, we should work to improve our capacities for operationalizing it, if at all possible. Hence, the problem is less the immediate difficulties in research than whether there are any reasons for believing that the nonissue is intrinsically unresearchable. Wolfinger, Merelman, and others seem to believe that there are. On the other hand, Bachrach and Baratz, despite their apparent rejection of the need for objective measurement, actually strive to spell out an "approach" to the empirical investigation of nonissues.[41] I agree with Wolfinger that the Bachrach and Baratz suggestions are inadequate and that researching nonissues is difficult; but I disagree that it is intrinsically impossible. I agree with Bachrach and Baratz that nonissues are a vital aspect of politics and with their argument that they are researchable; but I do not find their research suggestions fully satisfactory. What now follows is an attempt to evaluate the main criticisms leveled against the researchability of nonissues, plus a very general indication of how such research should proceed. Since Wolfinger's article deals at length with most of the specifics of the Bachrach and Baratz approach and also develops the major objections to it, I shall assume familiarity with those arguments and not recapitulate them here.

As I have said, the two crucial elements of the nonissue are expectation and prevention. The analyst of the nonissue must first determine and justify his consideration of certain nonevents (failures to attempt influence) as possibly indicative of power relations. Then he must be able to show empirically that the failure of some actor (elite or nonelite) to attempt influence was significantly caused by some other actor in the system rather than being an autonomous decision. Beneath the polemics, there seems to be considerable implicit agreement on this point by both "neo-elitists" and "pluralists."

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[40] P. 952.

[41] "Decisions and Nondecisions . . ."; *Power and Poverty*.

Expectations of Influence. How is the analyst to develop a useful and plausible set of expectations about the probable influence attempts of the main actors in the political system being scrutinized? He has several important tools for this, though neither individually nor collectively are they fully developed. I should urge, however, that we not adopt as our motto, "Why let things be difficult when, with just a little more effort, we can make them seem impossible."

Foremost among the tools for determining expectations regarding influence attempts by various actors is *theory*. What is obviously needed is a psychology of power—some fairly reliable body of knowledge about power motivations and related cognitions, as they lead to attempted influence. We need to construct a reasonably general understanding of what leads various types of political actors to protest, rebel, vote, bargain, command, connive, threaten, promise, persuade, and so on, through the basic repertory of politically significant influential behaviors.

Wolfinger remarks the importance of "implicit assumptions" and even of "deductive models," though he does not pursue this line of thought very far. He is quite correct in observing that the relevant theory we now have is "rudimentary," but it is not totally lacking. I must grant that, at the individual level, it is almost amazing to me that political science as a discipline has not only done so little toward fathoming people's power orientations, but that it has *attempted* so little. Despite Harold Lasswell's early work and compelling exhortations, systematic empirical efforts to assess individuals' "power drive" (their desire for power of various sorts), "power styles" (their repertories of influential behaviors, among other things), and what I should call "power salience" (the relative degrees of attention they pay to power phenomena) have been almost nonexistent.[42] Moreover, the few that do exist have come much more from social psychologists than political scientists.[43] The study of "political participation," the current vogue of political scientists, seems to me trapped in a rather narrow focus on a few quasi-formal activities such as voting, party membership, office-holding, campaign work, etc.

At other levels, however, we have a good deal of fairly crude theory about the wellsprings of interest group activity or the sources of governmental action (including conscious inaction), as Dahl pointed out in his reply to Walker.[44] These scattered nuggets need to be reintegrated under a more comprehensive theoretical perspective, but even now it is possible in limited areas for us to present many rough but useful hypotheses about how the various actors in a community will try to exert influence. Existing theory of this type may not be sufficient to make an open-and-shut demonstration that no plausibly expected influence attempt has been neglected; hence, complete protection in advance against charges of a hidden element's influence through non-decision making may be impossible. Those making such charges, however, as Wolfinger indicates, must themselves specify the kinds of influence attempts which have presumably been overlooked. They should also give some fairly cogent, albeit hypothetical, indication of the power mechanisms by which these expected influence attempts were deterred. It is as easy to charge hidden factors as it is to sue for damages. In either instance, to avoid a prompt dismissal, the plaintiff must present clear presumptive evidence.

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[42] Harold D. Lasswell, *Psychopathology and Politics* (New York: Viking Press, 1960), esp. Chapter IV, pp. 38–64, and *Power and Personality* (New York: Viking Press, 1962), *passim*. Cf. Janowitz' comment that "It is no longer fashionable to speak of the psychopathology of politics, but the issues that Lasswell raised a quarter of a century ago about the unconscious motives of politicians remain unclarified and unprobed, especially in community political systems," "Community Power and 'Policy Science' Research," p. 402.

[43] For example, David C. McClelland and his students, James Uleman and David Winter, working on "*n* power." See, McClelland, *The Achieving Society* (Princeton: Van Nostrand, 1961), pp. 167–170; James S. Uleman, "A New TAT Measure of the Need for Power," unpublished Ph.D. dissertation, Harvard University, 1965; David G. Winter, "Revised N-Power Scoring System," unpublished manuscript; Joseph Veroff, "Development and Validation of a Projective Measure of Power Motivation," *Journal of Abnormal and Social Psychology*, 54 (January, 1957), 1–8. Also note Richard Christie and Florence L. Geis, *Studies in Machiavellianism* (New York: Academic Press, 1970); T. W. Adorno *et al.*, *The Authoritarian Personality* (New York: Harper, 1950); as well as the standard "dominance-submission," "power," and "autonomy" components of the basic personality inventories. One rare attempt by political scientists to investigate individual power motivations in this fashion is Rufus P. Browning and Herbert Jacob, "Power Motivation and the Political Personality," *Public Opinion Quarterly*, 28 (Spring, 1964), 75–90.

A quite provocative viewpoint also comes from some of the post-Freudian psychoanalysts such as Karen Horney, Harry Stack Sullivan, and Erich Fromm. All in all, however, there seems to be a tremendous imbalance in the study of the psychology of power which concentrates on the person influenced and what leads him to be influenced much more than on the influencer and what leads him to attempt influence. Interestingly enough, the analogous imbalance seems present in the field of communications research.

[44] "Further Reflections . . . ," pp. 304–305.

In addition to developing expectations of influence attempts from available theory concerning power motivations and perceptions, the researcher usually must also conduct empirical investigations to learn whether the theoretically predicted orientations were actually present in the actors involved. Reference group theory and notions of relative deprivation, for example, in conjunction with other data, might induce one to expect discontent leading to a certain type of influence attempt by a particular group. This theoretically formed expectation should then normally be buttressed by empirical evidence of such discontent and of the psychic association between it and the urge toward the expected influential behavior. The theoretical expectation provides a specific focus for the empirical investigation. It is true that unconscious phenomena are always difficult to research and may be important for nearly any political investigation, not only power analyses. But sophisticated research following clear theoretical guidelines can take us very deeply into those orientations which are psychically more available, as *Who Governs?,* Barber's *Power in Committees,* Neustadt's *Presidential Power,* Banfield and Wilson's *City Politics,* and other works reveal.[45]

In all these matters we cannot now operate with great elegance and precision. But I see no insurmountable impediment confronting our ability to become much more effective in the future if we put the resources into the effort. Wolfinger objects that failing to find a predicted influence attempt might mean either that a nondecision had occurred or that our theory was wrong. Quite true. But this is by no means a special hardship. All cumulative science is tentative for this reason, since additional work is based upon accepted earlier theory which may prove wrong. The early stages of a science are especially hard because the theory on which to erect more theory is meager and frail. But there is no alternative. Actually, as I shall argue again in a moment, the power analyst is not so terribly ill-positioned in this respect. If the theory which permits him to identify an expected influence attempt that did not materialize is wrong, then he should be unable to complete the second necessary step of his investigation into the nonissue, namely, determining how some identifiable actor forestalled the expected influence attempt—by creating anticipations of negative reactions, by inculcating values or perceptions which deterred action, and so on.

The second major tool for developing plausible expectations about influence attempts is explicit empirical *comparison.* For example, of seven communities having very similar air pollution levels and matched along several other dimensions, if six display popular protest movements about pollution while the seventh does not, one would be led to look for possible nonissue phenomena in the seventh city.[46] If virtually all groups within a given system which have suffered a sharp decline in perceived status respond by partisan political activity but two do not, then one is led to look for latent power processes which might possibly have deterred such action. Wolfinger notes that strictly controlled comparison is rarely possible for the communities of interest in the natural world. Too many factors vary simultaneously. But that also is one of the awkward facts facing most social analysis. Informative, though less than decisive, comparison is still possible and important. Political science has made such lamentably little use of experimentation that most of our knowledge has come through this less rigorous but still useful route of comparison that is only partially or grossly controlled.

Along these lines, another avenue of investigation seems promising. Most writers acknowledge some intuitively clear cases of nonissues, such as the "false consciousness" of the southern Negro referred to by Wolfinger. Others might add different matters such as women's opportunities in society, the treatment of American Indians, preservation of the environment, etc. Careful, theoretically informed study of these few presumably manifest cases of nonissues might be most useful in helping develop theories about the conditions under which various types of issues are likely to be suppressed, intentionally or otherwise.

As another tool for forming expectations about influence attempts, Bachrach and Baratz urge "inquiry into which persons or groups, if any, gain from the existing bias and which, if any, are handicapped by it."[47] Here, however, one must be quite careful in making inferences about power based on existing distributions of values, as both Polsby and Wolfinger have cautioned.[48] Unintended and unrecognized influence, side effects, and other factors than power which also affect who gains and who loses, make these inferences erroneous in many circumstances. Nonetheless, I concur with Wolfinger that they constitute a useful clue—a tentative first insight—into relative distributions of power. We can suspect nonissues when: 1) glaring inequalities occur in the distribution of things avowedly valued by actors in the system, and 2) these inequalities do not seem to occasion ameliorative influence attempts by those getting less of those values. Such suspicion of a nonissue may prove correct or false, depending always upon further empirical confirmation, especially through more concrete evidence that the initially inferred values are actually held and through location of the power mechanisms by which their expression has been suppressed.

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[45] James D. Barber, *Power in Committees* (Chicago: Rand McNally, 1966); Edward C. Banfield and James Q. Wilson, *City Politics* (Cambridge: Harvard University Press and the M.I.T. Press, 1963); Richard E. Neustadt, *Presidential Power* (New York: Wiley, 1960).

[46] Cf. Matthew Crenson, *The Un-Politics of Air Pollution: A Study of Nondecisionmaking in the Cities* (Baltimore: The Johns Hopkins Press, 1971).

[47] "Two Faces of Power," p. 952.

I agree that current research on policy outputs may contribute to a marked advance in our ability to determine value distributions and their relationship to certain governmental forms. I disagree that this field has "gotten by" or will ever be able to get by with virtually no discussion of power and nonissues. It seems to me that community power analyses and policy output studies are essential to each other. Power has intrinsic interest, but a major concern is obviously how different power arrangements affect the creation and distribution of other values. The distribution of outputs has intrinsic interest, but usually one cannot satisfactorily explain variations in such distributions without taking profound account of patterns of power. The two research efforts suffer in separation and must be linked.

Preventive Power Mechanisms. Discovering, if possible, a set of plausibly expected influence attempts which never materialize is but the first part of researching nonissues. A perhaps easier but no less crucial phase of the process is the subsequent effort to locate for each unrealized expectation the power mechanisms of suppression which establish the existence of a nonissue rather than an autonomous determination. This second phase naturally cannot ensure that all nonissues have been unearthed; but it does constitute the main protection against spurious inclusions, ensuring that all nonissues accepted are genuine.

In general, discovery of the power mechanisms used for preventing influence attempts proceeds through the usual techniques for locating and measuring power, including attributions by participants, reconstructive decisional analysis, direct observations, and so on. Dahl, Merelman, and Bachrach and Baratz all make this point.[49] I shall not discuss these methods for locating and measuring power relationships further; they are fairly well understood. A few particular points, however, need brief mention.

One of the most basic distinctions among these various suppression mechanisms involves a key aspect of any power relation—recognition. Some of the suppression mechanisms will be recognized by both the influencers and those influenced. Blatant threats such as, "If you dare raise that matter you'll be fired," are an example. Some are recognized by only one party, either the influencer or the influencee. And, at the other pole are those suppression mechanisms recognized by neither party. These are likely to be relationships so thoroughly woven into the culture that all participants take them for granted. The last are obviously quite difficult to uncover, though the best hope lies in a cross-cultural perspective.

The most conspicuous practical research approach into nonissues suggested by Bachrach and Baratz deals mainly with the more unrecognized situations. They propose investigation of ". . . the particular 'mobilization of bias' in the institution under scrutiny." This requires analysis of the "dominant values, the myths, and the established political procedures and rules of the game."[50] In short, they advocate analysis of the power consequences for influence attempts of the prevailing political culture and institutional framework.

Bachrach and Baratz are quite vague, however, about how this is to be done. Perhaps the essential point here is that merely showing that there are dominant values and beliefs or institutional arrangements which differentially enhance the power of some actors and hinder others (mobilize bias) does not indicate a nonissue, as previously defined. One must show further that those currently prevailing belief systems and institutional arrangements are the result of influences by specifiable actors in the system, rather than the result of largely autonomous processes. The situation often gets complicated because beliefs and institutional arrangements which foster the power of some actors over others may be perpetuated by still other actors and be unrecognized and remote from those whom they most advantage. In this last instance, a nonissue is present, but it may not be the one superficially concluded.

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[48] Polsby, *Community Power and Political Theory*, p. 132.

[49] Dahl, "A Critique . . . ," p. 469; Merelman, "On the Neo-Elitist Critique . . . ," p. 453; Bachrach and Baratz, "Two Faces of Power," p. 952. In fact, in "Decisions and Nondecisions . . . ," Bachrach and Baratz seem to hint at the type of argument I am making regarding the two phases of research on nonissues: establishing expectations regarding influence attempts and then finding the power mechanisms for preventing influence attempts. They say, "although it is true that a nondecision is not visible to the naked eye, a latent issue is discernible and so is the mobilization of bias. Thus it can be said that the *nondecision-making process* (the impact of the mobilization of bias upon a latent issue), in distinction to a nondecision, is indeed subject to observation and analysis." McFarland also seems to adopt a position similar to mine regarding establishing expectations of influence attempts through theory and comparison: "A partial solution to this problem may be achieved if the observer (1) explicitly states assumptions concerning all human behavior, or (2) utilizes a comparative method," *Power and Leadership . . . ,* p. 76.

[50] "Two Faces of Power," p. 952.

An educational system, for example, may teach values which aid the power of an elite stratum. The teachers and educational officials inculcating such values may not be of that stratum themselves and may not profit in any clear way from the situation. Moreover, there may not be any chain of influence through which the benefited elite can be shown to control the educational system. On deeper analysis one might find that the teachers and officials act rather independently in this respect, gaining psychic gratification from identifying with the elite rather than being subject to any apparent direct or indirect influence from the elite. Of course, one can always contend that the fabric of the social structure itself produces this tendency toward identifying with high status elements—in fact, produces the very stratification system itself. But one would then still have to show that this elite was responsible for the social structure and stratification system. This can be a very awesome task indeed.

On the other hand, there seem to be many instances in which the mobilization of bias is much more easily discovered. In these cases, the researcher would try to determine which elements of the prevailing political culture and which institutional arrangements were significantly related to the limited issue-area or activity he was investigating. As Dahl attempted in New Haven, Banfield attempted in Chicago, Vidich and Bensman attempted in "Springdale," and Bachrach and Baratz attempted in Baltimore, he must strive to spell out the salient features of the predominant belief systems related to the influence activities he is investigating. Such analyses refer to cultural norms like legality, the avoidance of violence, a concern for governmental economy, preferences for or dislike of certain verbal styles and physical appearances, the legitimacy of business as an essential and proper institution, the acceptability of compromise, and so on. As Dahl remarks, ". . . the belief system has precise injunctions and vague mandates; and it is chock-full of inconsistencies."[51] Frequently many belief systems are involved, rather than merely one. But, in Dahl's words again, "there are certain tacit understandings in the local political culture that sophisticated participants can hope to rely on."[52] The analyst of power concerned about nonissues must examine those tacit understandings which seem to bear upon the particular scope he is investigating. He must first determine what they are, and then ascertain how they are established, at least sufficiently to know whether they can be attributed to the influence of particular actors in the political system he is analyzing.

The same kind of analysis must be applied to institutional arrangements. Particular institutional patterns, formal and informal, may advantage some actors and disadvantage others in their exercise of power. Voting schemes offer the most familiar example, but numerous others come to mind, such as access to information, time schedules, the size of decisional units, property rights, etc. The effect of such institutional factors on power in the scope being investigated must first be ascertained, and then one must determine the extent to which those arrangements can be said to be the result of the power of identifiable actors in the system. The analysis is predicated on the assumption that not merely the origination of prevailing belief systems and institutions may be the result of the exercise of power, but also their maintenance, once established. At the same time, such beliefs and institutions may be the result of autonomous processes or of influence patterns so diffuse that they defy attribution to specific actors, elite or other. In these cases, the notion of the nonissue, as defined, is not applicable. The mere fact that beliefs or institutions aid some actors is not proof of the existence of nonissues or covert power.

The same type of analysis, incidentally, applies in the case of anticipated reactions. One must first discover the relevant anticipations. These may or may not be the result of some exercise of power. If they are, then it is essential to discover who created the anticipations. It may or may not be the actor whose behavior is being anticipated. The actors inducing the anticipations constitute the true influencers. Occasionally there may be an extremely complex reciprocal power relationship such that all actors are simultaneously creating and reacting to anticipations created by others. In such instances, the analyst may not be able to proceed beyond establishing the reciprocal linkages—i.e., he may not be able to decipher all the individual causal flows. In general, however, anticipations, like failures to attempt influence, may be essentially self-generated or the result of extremely diffuse processes, so that every instance of anticipated reactions altering behavior is not necessarily a power relationship.

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[51] *Who Governs?* p. 84.

[52] *Who Governs?* p. 80.

Objections to the Researchability of Nonissues. I have adumbrated a basic approach and several very broad-gauged tools for researching nonissues. In passing, I have commented on many objections raised by Wolfinger and others. In concluding this topic, let us review the two major types of objections leveled against the researchability of nonissues—subjectivity and impracticality, viewing them against the approach suggested.

Wolfinger quite justifiably says that "the core of the problem is the difficulty of identifying nondecisions, which seems generally to come back to determining people's 'real interests.' . . ." He avers that ". . . what is a nondecision depends on one's beliefs about how well contemporary political discussion and action match the ultimate political good." I agree that this has been a danger. But I believe that the approach specified above largely eliminates this tendency. Nonissues *can* be researched as objectively as other established topics in political science, and researching them is vitally important. The necessary expectations of influence attempts can be openly and relatively objectively developed, and the process is anchored by the second-stage determination of the specific power relations involved in deterring expected influence attempts. The effort is conservative in the sense that some nonissues may be missed, but all identified will be genuine. We must be concerned lest subjective, ideologically generated attributions of "real" group interests, masquerading as objective or scientific findings, become the quicksand on which we try to base scholarly power analyses. My argument is that this is not necessary, that respectable alternatives exist, and that the topic is so crucial that great effort to resolve existing difficulties is warranted.

After subjectivity, the main objection raised by critics concerns practicality—the vastness of the research demands. This objection is, I think, more telling. Some facets of the nonissue are certainly complicated and elusive. Similar depth in scrutinizing most other important areas of political research, however, would unearth similarly imposing demands. The critical point is that none of the demands be unrealizable in principle, and they are not. Moreover, many of the suggestions made here and by Wolfinger would act to reduce these problems. It is not necessary, for example, to consider ". . . all those alternative courses of action that politicians might adopt from which they choose one." A focus on empirically developed expectations of influence attempts reduces this burden enormously. Also, the narrower and sharper focus of power research on specific, well-defined scopes, along with the suggested refusal to generalize to "*the* power structure of the community," greatly lowers the magnitude of the research task. Besides, as Wolfinger underlines, we are aided by the fact that politicians, like everyone else, do not consider *all* alternatives, but instead make "successive limited comparisons." Theoretical and conceptual refinement, aided by some further economies furnished by reality, would seem to make researching nonissues manageable, so long as one does not demand early completeness in findings. The research will be difficult and probably slow; but it seems possible, promising, and essential.

Conclusion

A minor but significant theme in what has been said implies that basically the nonissue is even broader and more important than "renunciation," "abstention," and "nonparticipation." It involves much more than keeping things off the agenda through anticipated negative reactions, false consciousness, or threatened force. In two ways most prior discussions of nonissues have stressed the negative. I should like briefly to "accentuate the positive" in order to suggest a more complete picture.

The first positive accent is interpretive. Earlier I noted the importance of anticipated positive reactions as well as negative, and referred to a mirror-image of the nonissue: in addition to the suppression of expected influence attempts we must also consider the stimulation of unexpected influence attempts. Both are part of the overall picture of power being used by some actors to affect the power attempts of other actors, sometimes to deter them and sometimes to encourage them. In fact, power is used not only for these two purposes but also to shift contemplated influence attempts from one form to another. Along with the "nonissue" (better labeled "suppressed issue") we must consider what I shall call, for want of more appropriate terms, the "stimulated issue" and possibly the "shifted issue."

The second positive accent is evaluative. In general, the nonissue and nondecision seem to be regarded as unfortunate. Covert power is usually deemed bad. I think, however, that further investigation will require a more discriminating verdict. At times there would seem to be great individual and social utility in some kinds of nonissues. For example, as part of its capacity for "interest aggregation," every durable social system has to say no to various actors. On saying no, it must face the consequences of what the denied actor will do next. Ordinarily, it possesses formidable "cooling out" processes whereby the disappointed are led to accept their lot, at least temporarily, without disrupting the system too severely. Such processes can, of course, be insidious. But they also offer important benefits, depending on how they are used. Every issue which agitates some actor in the system cannot be taken to the pinnacle of decision making nor become the focus for a conspicuous power struggle. Mechanisms to put an early and inconspicuous *quietus* on some potential issues are probably essential to every large political system. The question is more a matter of how to decide what shall be nonissues than of doing away with them entirely. Similarly, it is but a step from false consciousness to salutary consensus—a step that rests upon a moral foundation. Indeed, use by a community or society of socialization processes to achieve certain values almost always has a disturbing aspect as well as a desirable one. The nonissue, like most basic political phenomena, has both positive and negative possibilities, depending on many contextual and specific factors.

Power Structures. At the conclusion of his article, Wolfinger includes what seem to me a few *obiter dicta* regarding the concept of power structures. Since it is by no means possible to treat this topic fully, I should probably not even broach it here. Nevertheless, since several "pluralists" have made similar assertions from time to time, I shall conclude with a few *obiter dicta* of my own.

A power structure of a political system has two fundamental components: 1) the set of all actors distinguished in the system, and 2) a set of power relationships mapped onto those actors. The power relationships refer to some specific time period and, minimally, to some specific scope or influential behavior. A more complete specification will include both scope and influential behavior. In a large community, most of the actors would be groups, organizations and strata. As with all political analysis, fruitful designation of the major actors, the scopes, and the time period, is extremely difficult. Concern for power structures exposes these difficulties in a very chastening fashion.

So far, there is no disagreement. Wolfinger, however, asserts that "attempting to describe a power structure also presupposes that one can attribute power to actors so that the power of different actors can be compared, and so that their power can be aggregated in order to estimate the relative power of different coalitions." This is rather dubious. More accurately, describing a power structure presupposes that one can determine, within a given system, who influences whom with regard to a specific scope by doing what. Domains and power fields (that is, the sets of actors influenced by and influencing any given actor of reference) can be compared, mainly in terms of their sizes. But comparison of the power of different actors may mean much more than that. If so, it is not necessarily implied by the power structure. Such matters as resistances and costs must be taken into account to proceed further.[53]

The inference to the power of different coalitions is also by no means certain. If the power structure accurately portrays power relations in the system, then some inferences to coalitions can be made. But if the coalition implies the presence and recognition of a *new* actor in the system, then no easy inference is possible. To permit a valid inference, the coalition must have been included as a specified actor in the original power structure and in the data gathering on which it is based.[54]

Wolfinger raises three other objections to the concept of power structures. The first is the problem of indirect influence, especially anticipated reactions. It must be remembered that we are talking about a structure of actual, past or present power relations, not potential power relations. As I have previously indicated, anticipated reactions present no unusual problem in this case. Both anticipated reactions and indirect influences do demand rich and voluminous data which are hard to obtain. But the difficulties do not vitiate the utility of the notion of power structures.

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[53] On these concepts, see the articles referred to in note 4.

[54] For example, suppose we single out two individual actors in the structure (actors A and B) and note the set of actors influenced by each (i.e., their domains). Suppose also that in the domain of each there are actors who are not in the domain of the other. Now suppose that we consider these two actors as a coalition (AB). It does not necessarily follow that we can simply add their individual domains to get the domain of the coalition. At least some of the actors in one of the individual domains might react very differently if they perceived they were being influenced by the coalition AB rather than by actor A or actor B alone. Much will depend on whether this coalition is recognized by other actors and, if so, what their attitudes toward it will be. One can be sure of this only if data have been gathered regarding the new actor in the system, the coalition AB.

The second objection asks how power is to be attributed to political innovators, who focus attention and define issues. It is directly analogous to asking how power is to be attributed to actors who suppress influence attempts. The answer is through appropriate specification of scopes and influential behaviors. These people are exerting particular types of influence. They should appear as extremely potent when those types and scopes are presented. They may not be powerful in other respects.

The third objection essentially raises the subtle and somewhat thorny question of when a power relationship is to be regarded as terminated. The basic answer is: when there is no longer any *change* in the behavior of the influencee that is produced by the behavior of the influencer. Once the 21-day rule is established in Congress, that specific influence is finished. Congress has an established new character in which it persists without further influence by those who put through the 21-day rule. If, however, maintenance of the 21-day rule requires constant influence by the original influencers—if Congress would otherwise lapse back into other procedures—then the power relationship persists rather than terminates. Clarity regarding the criteria for asserting the existence of a power relationship is, of course, essential to the conception of a power structure. But the threat to that clarity from the termination problem can be satisfactorily resolved.

Finally, in this all too skimpy reply to all too skimpy charges, I think it important to register an objection to another assertion of some "pluralists" regarding power structures. The assertion is that the conception of a power structure is invalid because it assumes a static distribution of power. In a trivial sense this is true. The power structure does depict a fixed set of relations for a given time period. But in a deeper sense it is false. The analyst describing the power structure must establish explicit criteria for concluding that a power relation existed between two actors during the period. If the period is too long, so that significant change in power relations occurred during it, he is likely to have a hard time developing such criteria. At one point X will have influenced Y, but at another point no such influence will be found. The conscientious researcher will then either indicate this fact in describing the power structure or, more likely, reduce the length of the period to obtain more stability in the power relations described. Thus, a situation of change will be depicted by several power structures. The periods can be as short as necessary to accommodate the data. In fact, the existence of the change and its precise nature is most suitably revealed by thinking in terms of power structures. Moreover, if one wishes to become more sophisticated, probabilistic representations of power relations, notions of dynamic structures, moving equilibria, and the like are available for even more adept handling of change.

In general, all discussions of power distributions, dispersed inequalities of power, even democracy and dictatorship, imply thinking in terms of power structures. Sociometry, systems analysis, and many core notions of political science are predicated on such a concept. Like the nonissue, the notion of power structures is basic and workable, though difficult and needing further exploration.[55]

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[55] It is sometimes also suggested that the connotations of the term "power structure" lead one to assume that ". . . power and the class or status structure of the community are linked in a certain way (i.e., the way the stratification writers describe) . . ." Polsby, *Community Power* . . . , p. 97. On this I can only say that I see no such connotations personally or among political scientists. The linkages or correlations among power structures themselves (across issues or scopes) and between power structures and other social structures (such as communication, prestige, friendship, liking, wealth) are clearly a matter for empirical determination. They constitute a most important topic for investigation—a topic that eludes us without the notion of power structures.

Wolfinger also mentions that the notion of a power structure is "inherently comprehensive." It implies the inclusion of all actors and all relationships, given the social unit, the scope, and the time period. This is true, and it is probably also usually true that for scopes including many nonissues it may be uncertain whether *all* relationships have in fact been covered. But this is simply to say that the accuracy of the depicted structure may in some cases be plainly tentative. So may many other aspects of community research. It does not seem sufficient reason to discard an otherwise fruitful concept.

Rejoinder to Frey's "Comment"

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Professor Frey and I seem to be in agreement on several points: (1) The research procedures proposed by Bachrach and Baratz are unsatisfactory. (2) Worrying about criteria of issue selection is unnecessary; policy formation can usefully be studied issue by issue. Indeed, I would add that typologies of issues are one of the more promising developments in the study of politics.[1] (3) The notion of nondecisions is not a club with which to belabor *Who Governs?* in particular or "pluralists" in general. Frey has performed a considerable service by rescuing the idea of nondecisions from the ideologically tinged context in which its advocates generally have discussed it. (4) Analysts of policy formation who limit their attention to overt conflict miss many exercises of power. (5) The pluralist-elitist dichotomy is not a useful distinction.

The last two points call for further discussion. I do not know of any researcher who has disputed the fourth point. In his study of New Haven, Dahl employed three indices of power, "of roughly the same strength." One of these was: "When a proposal initiated by one or more of the participants is adopted without opposition."[2]

While Frey and I reach the same conclusion about the value of the pluralist-elitist distinction, our views on this subject are otherwise disparate. He feels that the debate has generated a number of valuable ideas but has become ungenerous and polemical and therefore should be ended. I think that "pluralism" has become so protean a word that it should be retired from serious discourse on grounds of excessive ambiguity.[3] More fundamentally, the pluralist-elitist dichotomy occupies a limited and dull segment of the subject matter of political science. Most interesting and controversial topics seem not be subsumed under this tired rubric, and knowing whether a particular political scientist is considered a "pluralist" or an "elitist" does not help classify him with respect to these issues. For example, Dahl and I, both considered pluralists, have expressed diametrically opposite views on ethnic politics, while Michael Parenti, a certified antipluralist, has embraced my position on the subject.[4] Nelson Polsby was the first scholar to offer a political interpretation of the sources of support for Senator Joseph McCarthy, thus challenging the speculations of a collection of largely pluralist writers. His position was later developed by Michael Rogin, who said that the position taken by Polsby's targets was an error of pluralism.[5] In other words, calling a political scientist a "pluralist" does not say much about him. In view of Frey's belief that the study of power structures should be at the center of political science, he probably would not subscribe to this dismissal. I will return to this issue later.

Frey and I differ on the central issue in our articles, the utility for research of the idea of nondecisions. His suggestions for research on this subject are vulnerable to many of my criticisms of Bachrach and Baratz's proposals, yet he does not respond at all to most of the problems I mentioned, and where he does respond, he is not always persuasive. He sees "two crucial elements" of research on nondecisions: the researcher must identify nonevents, and he must show that these are caused by "some other actor in the system." In order to ascertain the existence of nonevents one must first establish reasonable "expectations of the probable influence attempts of the main actors" in the political system. I argued that this was impossible except by introducing normative considerations. Frey proposes two objective approaches to the problem, both of which I consider inadequate. His first proposal is that plausible expectations of influence attempts can be derived from existing theoretical formulations. Having said this, he concedes that theory about what leads individuals to try to exert political influence is "almost nonexistent." But he says that the sources of interest group and governmental action are explained by "a good deal of fairly crude theory" that permits "many rough but useful hypotheses about how the various actors in a community will try to exert influence." Unfortunately, Frey does not provide examples of either theory or hypotheses. The only apparent support he gives for his claim is his footnote 44. This cites a passage by Dahl that also does not contain theories or hypotheses. In fact, Dahl, too, calls for hypotheses and theory. In short, Frey provided no examples of useful theory from which one could predict political activity at any level of behavior.

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[1] For the best known and most influential issue typology see Theodore J. Lowi, "Distribution, Regulation, Redistribution: The Functions of Government," in Randall B. Ripley, ed., *Public Policies and Their Politics* (New York: W. W. Norton and Co., 1966), pp. 27–40.

[2] Robert A. Dahl, *Who Governs?* (New Haven: Yale University Press, 1961), pp. 332–33.

[3] Lists of definitions of "pluralism" are given in Nelson W. Polsby, "'Pluralism' in the Study of Community Power, Or, *Erklarung* Before *Verklarung* in *Wissenssoziologie*," *The American Sociologist*, 4 (May, 1969), 118–22; and in my *The Politics of Progress* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1972), Chap. 1.

[4] *Who Governs?* pp. 34–36; Parenti, "Ethnic Politics and the Persistence of Ethnic Identification," *American Political Science Review*, 61 (September, 1967), 717–26; Wolfinger, "The Development and Persistence of Ethnic Voting," *American Political Science Review*, 59 (December, 1965), 896–908; and Wolfinger, "Some Consequences of Ethnic Politics," in Harmon Zeigler and M. Kent Jennings, eds., *The Electoral Process* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1966), pp. 42–54.

[5] Polsby, "Toward an Explanation of McCarthyism," in Polsby, Robert A. Dentler, and Paul A. Smith, eds., *Politics and Social Life* (Boston: Houghton Mifflin Company, 1963), pp. 809–24; Daniel Bell, ed., *The New American Right* (New York: Criterion Books, 1955); and Michael P. Rogin, *The Intellectuals and McCarthy* (Cambridge: The MIT Press, 1967).

In discussing the deficiencies of predicting behavior by theories specifying how groups or individuals should act, I said that if the predicted behavior were not observed, one could conclude either that a nondecision had occurred or that the theory was wrong. Frey brushes aside this objection by claiming that it is common to scientific endeavor, "since additional work is based upon accepted earlier theory which may prove wrong." This misses my point, which was that *one could not determine if the theory was wrong*. An absolutely essential desideratum of any scientific theory is that there must be clear ways to tell if the theory is invalid. Using unverified theory in the way proposed by Frey would violate this canon of scientific method.

Frey's "second major tool for developing plausible expectations about influence attempts is explicit empirical *comparison*." Once again, Frey furnishes no examples, but I imagine that this procedure would yield statements to the effect that in conditions X and Y the likelihood that group Z would make an influence attempt would be .65. Knowing this, and seeing conditions X and Y in his research site, what does a scholar conclude if group Z fails to make an influence attempt: that a nondecision has been found, or that his city falls into the substantial proportion of deviant cases?

The previous paragraphs touch on one of the more important drawbacks of the concept of nondecisions: Each form of nondecision resembles something else, so how can the researcher distinguish nondecisions from other phenomena? How can the renunciation form of nondecision be distinguished from officials' limitation of their agenda in order to economize on time and energy when they make decisions? How can renunciation be distinguished from observation of lower priorities (especially since the priorities themselves may reflect anticipated reactions)? Bearing in mind the possibility that the difficulties of taking political action may be results of previous exercises of power, how can abstention be distinguished from apathy, pessimism, or preferences for nonpolitical activity?

Analogous to these problems of differentiation is the question of *a priori* criteria for the threshold that separates issues and nonissues. Frey suggests that equal rights for women was a nonissue before the current women's liberation movement. It would not be difficult, however, to find examples of agitation for women's rights in the pre-Women's Lib days. What distinguishes that period from the current scene? It is easy enough to say today that women's rights was a nonissue in, say, 1949; the trick is to figure out a way to discover it *in 1949*.

With some exceptions, then, Frey has failed to discuss the conceptual deficiencies of nondecisions. One area of difficulties concerns the problem of comparing the power attributed to different actors. Frey deals with this by denying that such comparisons are necessary to analysis of power structures. This assertion merely exchanges one set of drawbacks for another. To Frey, an adequate description of a power structure is the explication of "who influences whom with regard to a specific scope by doing what." Inasmuch as he considers anticipated reactions an important manifestation of nondecisions, I assume that he includes in this category expectations of the electorate's probable responses to various courses of action. Since politicians generally like to avoid offending anyone, it would appear that power could be attributed to many people on any issue on which politicians made calculations of voters' possible reactions. In such cases, anyone whose behavior helped "create the anticipations" would be deemed powerful, with no distinctions as to the weight of their power. Lists of "the powerful" would be both lengthy and meaningless.

Frey's position on the operational practicality of the notion of nondecisions might be paraphrased like this: "I admit that research on nondecisions is hard, although perhaps you have made it look harder than it really is. But we should not let these problems discourage us, for the subject is important." At various places in his article Frey says that nondecisions are an "absolutely vital area for investigation" and that research on them is "essential," "vitally important," and "crucial." If political scientists do not do research on nonissues, the discipline "is likely to be an arid enterprise indeed." The concept of power structures is even more important: "All talk about . . . democracy and dictatorship implies thinking in terms of power structures." Frey also argues that the term "community power" should include topics like the treatment of women in everyday life, granting of financial credit to individuals, and private economic decisions. Since the essence of power is causality, the study of power structures is a search for answers to the question: "Who is responsible for outcomes in X issue area in the political, social, and/or economic life of this city?" Adding the idea of nondecisions further expands the search to include explanation not only of existing reality but also of the exclusion of alternative patterns of sexual relations, credit granting, factory location, tax policy, park administration, and so forth.

Despite all his categorical imperatives, Frey does not say *why* research on nondecisions is so essential to the viability of political science that the progress of the discipline should be measured by our success in the empirical study of the subject. Theoretical formulations are not written in fiery letters in the sky; they are constructs devised by scholars to facilitate the growth of knowledge. The utility of a theory or concept should be judged by its predictive, heuristic, or taxonomic value. In my opinion Frey has not shown that the concepts of nondecision and power structure contribute anything toward study of the numerous discrete topics discussed in our articles: the consequences of different political forms, styles, and processes, the role of myths, the impact of political socialization, etc. These subjects are important, should be studied, and are being studied. The idea of nondecisions does not make research on these subjects more important or desirable, does not seem to have stimulated research on them, and does not enhance the quality or value of that research. In short, nondecisions is an *unnecessary* idea.

Frey calls power structure a "fruitful concept." My dictionary defines "fruitful" as "productive of results; profitable." After all these years, advocates of research on power structures (who seem more numerous than its practitioners) should be able to offer some results to justify this notion to which so much attention has been given. Their evident inability to do so suggests that "power structure" is another dispensable concept.

Oppenheim's Defense of Noncognitivism

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Political scientists often are eager to get on with their investigations without bothering too much about ethical dimensions of their discipline. Indeed it is often said that the making of moral judgments and the inquiry into their foundations are just the sorts of activities which have plagued the works of political thinkers in the past and have prevented political science from becoming more a *science* than an art. Some political scientists have denied that their works involve any evaluative claims at all, either implicitly or explicitly. Others have conceded that their selection of problems or, for example, their defense of democracy does involve certain normative judgments. Members of the latter group subscribe to the tactic of setting out frankly and explicitly their value judgments and then getting on with the empirical job. In either case many would be happy to avoid the task of assessing the nature and justification of normative claims. The sentiment is repeatedly expressed that this is a job best left for the philosophers. It is not always clear whether the motivation for such remarks is the belief that philosophers are better equipped for a task worth doing or the conviction that such inquiries are ultimately futile. In any case it is interesting and helpful to be provided with a political scientist's extensive investigation of attempts to justify moral principles, one which not only critically assesses the positions of past and recent moral and political philosophers but also defends a thesis about the fundamental moral principles. The investigation I am referring to is Felix Oppenheim's in his recent book, *Moral Principles in Political Philosophy*.[1] In the first half of the paper I discuss the central issue of Oppenheim's work, namely, his attempt to show the incorrectness of one important thesis about the status of basic moral judgments (cognitivism) and his defense of his own position (noncognitivism). In the second half of the paper I turn to some further claims Mr. Oppenheim makes in his attempt to undermine objections that suggest his view is irrational, nihilistic, and so on.

Mr. Oppenheim's main concern is with questions that are described, in the terminology of recent moral philosophy, as meta-ethical. What is the scope of meta-ethical inquiry? Let us review some familiar distinctions. The received opinion has it that it is one thing to make a moral or value judgment and quite another to make judgments *about* value judgments or *about* the meaning of ethical terms. To say "democracy is good," "abortion is sometimes right," or "war is wrong" is to make a value judgment. To say that 'good' means 'is conducive to human survival' is to make a descriptive or factual claim about the meaning of a word, on the assumption that the definition proffered is put forward as explicative and not as reformatory. This last claim is, then, a meta-ethical claim. It is an empirical claim *about* the meaning of an ethical word and not a moral judgment. It may be confirmed or falsified by empirical (in a broad sense) means.

To say that moral or ethical statements are neither true nor false or that they have no cognitive meaning is also to make a claim *about* moral statements, and is not itself a moral claim. This last sort of a claim is a claim primarily about the justification (or lack of such) of moral statements. Such claims are *meta-ethical* assertions as opposed to ethical or normative assertions. According to Mr. Oppenheim his study does not advocate any normative doctrine, ideology, or principle of politics; it does not make any political value judgments but speaks *about* them (p. 4). Oppenheim's focus is, then, on the meta-ethical rather than normative. This distinction was not commonly made prior to this century, and the particular meta-ethical view that Oppenheim seeks to defend was clearly identified as such and explicitly defended only as of the 1930's and 1940's, primarily in the work of that British exponent of logical positivism, A. J. Ayer, and in the more detailed examinations set forward by the American philosopher, C. L. Stevenson.

With regard to the meta-ethical question of the nature and justification of ethical principles, a broad division has been made between theories which maintain that moral principles are true or false and, therefore, "cognitively meaningful," and theories which deny this. The former are described as *cognitivist* theories and the latter as *noncognitivist*. Mr. Oppenheim, in the tradition of Ayer, Stevenson, and others, seeks to rebut the cognitivist view and establish the correctness of the noncognitivist view.

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[1] All references, unless otherwise noted, are to Felix E. Oppenheim, *Moral Principles in Political Philosophy*. Page numbers in the text of the paper refer to pages in Oppenheim. (New York: Random House, 1968).

A critical examination of Oppenheim's defense is warranted for two obvious reasons. First, there is some evidence for the correctness of his contention that noncognitivism is an assumption underlying most modern writings dealing with empirical political research. Regardless of the extent to which this is true, it is clearly one basic meta-ethical position that is held by many political scientists and one which is very attractive in view of certain obvious difficulties with the cognitivist position. Second, political theorists and others have leveled a serious charge against the noncognitivist view. They suggest that the noncognitivist view leads to or supports moral nihilism and, further, undermines the possibility of criticism of totalitarian and antidemocratic political processes. More dramatically, some critics contend that noncognitivism is a bedfellow of tyranny, and that those who defend it intentionally or unintentionally give aid and comfort to tyrannical political systems. Mr. Oppenheim is sensitive to such criticisms and seeks not only to show that the noncognitivist view is correct but also that noncognitivism is not "pernicious" in the manner suggested. He maintains, on the contrary, that there is no inconsistency in maintaining a noncognitivist view while denouncing tyranny. Mr. Oppenheim's program is, then, an important one in both a theoretical and politically practical sense. I will show, however, why noncognitivism has not been shown to be the most plausible meta-ethical position. Just which position merits that appellation remains, I think, an open question. To support this contention I will indicate a series of objections to Oppenheim's position, objections which he has neither undermined nor outflanked.

The Heart of the Dispute

Oppenheim's noncognitivist position consists of several theses. To identify them and examine them it will be useful to set forth his definitions of cognitivism and noncognitivism. He states:

> The meta-ethical theory of value-cognitivism affirms that valuational and moral statements are assertions about objective states of affairs and have, as such, cognitive status; that is, they are, and can be known to be, either true or false (p. 21).

At this point we should explicitly note three distinguishable assertions: (C_1) moral statements are assertions about objective states of affairs; (C_2) moral assertions are true or false; (C_3) moral assertions can be known to be true or false. It is unclear whether C_3 logically follows from C_2, or if C_2 could be true when C_3 is false. If the "can" in C_3 means only "it is logically possible that," then C_3 would appear to follow from C_2. This point is minor, however, and I shall leave it an open question. On the other hand, if "can be known" in C_3 means something like "there is a known method of showing them to be true or false," then it is not at all obvious that C_3 must be true whenever C_2 is. On this construal, C_3 would entail C_2, but C_2 would not entail C_3. It is clear that C_2 is more central to the cognitivist view than C_3 and, for that reason, deserves special attention. Showing that all cognitivist attempts up to the present fail to demonstrate C_3 is not sufficient to refute C_2. Hence it is possible to maintain the truth of C_2 and concede that Oppenheim succeeds in showing (whether or not he does) that cognitivist arguments have not demonstrated the possibility of moral *knowledge*.

To further clarify the dispute, let us now take note of Oppenheim's characterization of the noncognitivist position. He states:

> Value-noncognitivism as a meta-ethical theory may be summarized as follows: Basic ethical principles have no cognitive status; they cannot be *known* to be either true or false because they *are* not true or false; and they are neither true nor false because they do not affirm or deny that something is the case (p. 24).

Here we should observe that the account which Oppenheim regards as conflicting with cognitivism is set out in a way parallel to the characterization of cognitivism. Further, in the above description of noncognitivism let us identify and here label three theses:

N_1: Ethical principles do not affirm or deny that something is the case.
N_2: Ethical principles *are* not true or false.
N_3: Ethical principles cannot be *known* to be true or false.

Thus, N_1 contradicts C_1; N_2 contradicts C_2; and N_3 contradicts C_3.[2] Further, in the last-quoted passage it is clear that Oppenheim's positive argument has the following structure:

(i) If N_1, then N_2
(ii) If N_2, then N_3
(iii) N_1

(iv) Therefore, N_3

Whether Oppenheim wishes to focus on N_3 or N_2, it is clear that his argument asserts the truth of N_1, the claim that ethical principles do not affirm or deny that something is the case; as he also puts the point: they lack cognitive status. Let us suppose that premises (i) and (ii) are unexceptionable and that the argument is valid. Whether or not Oppenheim has provided us with a *sound* argument (whether or not he has provided us with a cogent argument for noncognitivism) depends upon whether N_1 is true. N_1 is the keystone in Oppenheim's noncognitivist arch. N_1 *may* be true, but our crucial question is whether or not Oppenheim has demonstrated that N_1 is true. So, it is to the point to ask further: What arguments does Oppenheim offer in defense of N_1?

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[2] That the ethical principles are "basic" I will leave tacit, as Oppenheim himself does at times. Further, he seems to use "moral statements" (or "assertions"), the expression used in his characterization of cognitivism, as interchangeable with "ethical principles," the expression used in the characterization of noncognitivism.

Since Oppenheim claims that N_2 logically follows from N_1, any evidence against N_2 is, if he is correct, also evidence against N_1. Shortly I shall make some objections to N_2 and, for the above reason, objections to N_1. Prior to that direct assault on N_2 and an indirect assault on N_1, I should like to call attention to difficulties with N_1 itself. It is of crucial importance to noncognitivism as Oppenheim presents the view. To repeat, N_1 is the claim that ethical principles do not affirm or deny that something is the case. Oppenheim appears to gloss this formulation by saying that the utterances of such principles do not count as statement-making utterances.

First, when we are told that ethical principles or their utterances do not make statements, we are owed, I think, some account of how one can distinguish between a sentence whose utterance makes a statement and one which does not. That is, Oppenheim appears to be using some criterion for the statementhood of sentences, some criterion by which one can routinely tell when a sentence (or "principle") affirms or denies that something is the case and when it does not. But Oppenheim implicitly rejects any criterion couched in terms of the logical or grammatical form of the sentence in question when he acknowledges that empirical *statements* and moral principles may be similar in this respect; e.g., both may have a "declarative" form. Possibly he thinks that the utterance of a sentence fails to result in a statement when by its utterance a feeling or an attitude is expressed. On this view *expressing* an attitude and making a statement (with the attached properties of being true or false, correct or incorrect, etc.) are mutually exclusive activities. The difficulties with this proposal I reserve for my discussion of N_2. In the end, it appears that Oppenheim offers us no explicit criterion by which to distinguish sentences which affirm or deny that something is the case (or whose utterance results in a statement).

Two points are relevant here. First, Oppenheim is confronted at this point with many of the formidable difficulties that are involved in the attempts of linguists and philosophers of language to formulate an adequate theory of meaning. Such a theory has not yet been devised, and any meta-ethical theory that presupposes its existence can hardly be well-founded. Second, if Oppenheim is correct in asserting N_1, then it is difficult to see how moral principles might compete or conflict with one another and also how they could form a set which is consistent or inconsistent. Ordinarily a set of sentences is said to be inconsistent just in the case where they cannot all be true at the same time and consistent where they can all be true together. Nevertheless, Oppenheim allows himself to say "We have seen . . . that the adoption of *some* consistent set of normative principles is a requirement of rationality . . ." (p. 161). How can he allow talk of consistency (which is defined in terms of possible truth) when in his view ethical principles cannot possibly be true since they do not, in his view, affirm or deny that something is the case? In addition, how can he consistently allow himself to talk of *conflicting* systems of political ethics or *conflicting* moral judgments when talk of "conflict," it would appear, must be cashed in in terms of inconsistency? (pp. 4, 171). More generally, how can an ethical principle ever clash with another when it neither affirms nor denies that anything is the case? Now, perhaps the notion of "conflict" can be explicated without reference to consistency (or inconsistency), or perhaps "consistency" can be analyzed without reference to truth or falsity, or affirmation or denial. It appears that imperatives conflict without affirming or denying, or being true or false. Still this possible strategy may or may not be successful. And if the acceptance of N_1, (and /or N_2) necessitates the sort of conceptual revision involved in surrendering our belief that ethical principles may conflict, that is a consideration against accepting N_1. If it does not necessitate such revision (as Oppenheim's discussion suggests), then we need to be shown in further detail why it does not. Perhaps it is unfair to expect such an account to be found in Oppenheim's book, but it does not seem unfair to require such an account before the verdict "acceptable" is passed upon noncognitivism. In view of the fact that N_1 is a central element in noncognitivism, these questions can hardly be relegated to the limbo of "finer points of the debate which are at best only of marginal interest."

Closely associated with both theses N_1 and

N_2 is Oppenheim's response to the question, "If moral statements neither affirm nor deny that something is the case, just what function do they perform?" By way of examining N_2 more directly, it will be to the point to assess a positive thesis of Oppenheim's, one designed to answer the above question. Oppenheim's familiar answer is: They express attitudes or feelings (pp. 27, 179). This last thesis Oppenheim rightly distinguishes from a variety of cognitivism, sometimes labeled "subjectivism," a view which construes moral principles as assertions *about* feelings or attitudes. On this last view, for example, "war is wrong" means "I have an attitude of disapproval toward war" or perhaps, "most people have a negative attitude toward war." Such claims *about* attitudes or feelings are empirical claims and, hence, true or false. Oppenheim presents the standard objections to this subjectivist view; our concern is with his expressive theory, one usually labeled as emotivism.

Just what is the relation between his positive thesis and his negative thesis, N_2? If one could maintain that (1) expressing an attitude or feeling by uttering a sentence and (2) saying something which is true or false are incompatible characteristics, then the truth of the positive thesis would provide grounds for concluding that the negative thesis is correct, i.e., that moral utterances do not count as statement-making utterances. But the former claim concerning incompatibility (as has been repeatedly observed in recent moral philosophy) suffers from serious objections; there does not appear to be any such incompatibility. For numerous familiar utterances involve true or false statements *and also* express feelings or attitudes (and, further, function so as to evoke feelings in others as well as influence their behavior). For example, in familiar contexts the following utterances may have both such "descriptive" and "dynamic" features:

a. Your wife is promiscuous.
b. War protesters are criminal misfits.
c. Students are effete snobs.
d. Governor Wallace is a racist.
e. This building is on fire.

Thus, the possible attempt to derive the truth of Oppenheim's negative thesis from the alleged truth of his positive assertions about the function of moral utterances appears to fail.

If this last judgment is correct, there is no necessity for us to examine the correctness of the positive thesis, but in any case that thesis poses serious difficulties also. Insofar as it asserts that the expression of attitudes or feelings is a *universal* feature of moral judgments, it does not deal adequately with unuttered private moral judgments. In light of the direct relevance that such considerations have upon Oppenheim's negative thesis, it is surprising that Oppenheim fails to discuss them. Of course he may believe that his negative thesis needs no logical support from the direction of his positive one or indeed that there is some other independent way of demonstrating the truth of his negative thesis, N_2, one quite independent of the positive thesis.

Mr. Oppenheim's procedure on this crucial point, however, is subject to the charge of simply assuming an answer to one important question. To support this suggestion I must call attention to the general structure of his mode of argument. Oppenheim exhibits the conflicting nature of the cognitivist and noncognitivist accounts in the way we have surveyed. He proceeds to attack two standard cognitivist attempts to show that moral principles are, and are known to be, true or false, namely, intuitionism and naturalism. If either of these attempts to show that moral principles are *known* to be true or false were successful, then it would follow that such principles are true or false, and Oppenheim's negative thesis (N_2) would be false. So it is important for Oppenheim to rebut these cognitivist attempts. Let us suppose that his arguments to this end are successful. Would it follow from the assumption that these cognitivist attempts fail that moral principles *cannot* be known to be true or false? It would follow *only if* the cognitivist arguments Oppenheim discusses exhaust the set of possible cognitivist attempts. But Oppenheim does not attempt to establish this required premise. Further, it is difficult to see how such a premise could be established. In any case, Oppenheim does not take steps in that direction. At one point he states, "It is one thing to commit oneself to some moral principle; it is another to claim that it is demonstrable" (p. 179). We might add the following maxim: It is one thing to show that some attempts to demonstrate the truth or falsity of moral principles fail; it is another to claim that they cannot be so demonstrated.

Mr. Oppenheim is not wholly unaware of this difficulty but he is insufficiently attentive to the criticism just made. He acknowledges that he has dealt explicitly with only a small number of cognitivist theories (p. 167). He goes on to say that others could "easily fit into one or another of our categories" (p. 167). His doubt that a radically different cognitivist theory (which evades his criticisms) may be developed is understandable and may turn out to be correct, but all this is no substitute for demonstration; after all, it is essentially Oppenheim's own complaint against the cognitivists: "You have not demonstrated it." With certain qualifications, then, Oppenheim concludes, "If cognitivism in general is false, then its denial, noncognitivism, must be true" (p. 167). Once more this simply will not do. For while Oppenheim may have shown that certain cognitivist positions are false, he has not shown that cognitivism in general is false. Furthermore, it is a basic point of logic that noncognitivism as he characterizes it is not simply the denial of his cognitivism. Each view, as he poses it, consists of a conjunction of three statements.[3] Cognitivism is the assertion of the conjunction consisting of C_1 and C_2 and C_3; noncognitivism is the assertion of the conjunction consisting of N_1 and N_2 and N_3. Given this analysis of cognitivism, one need only show that C_1 or C_2 or C_3 is false in order to show that cognitivism is false. If it could be shown, for example, that C_2 is false, then cognitivism, as here understood, would be shown false. From the falsity of C_2, however, it only follows that N_2 is true; it does not follow that the conjunction consisting of N_1 and N_2 and N_3 is true. Thus, it does not follow that noncognitivism would be shown correct. In short, on Oppenheim's explication of the two positions, noncognitivism is obviously not the same as the denial of cognitivism.[4] In the way indicated, Oppenheim, however, identifies or mixes what is distinct.

The Rationality Thesis

Associated with Oppenheim's defense of noncognitivism is another thesis I should like to examine; it might be called the rationality thesis. He maintains that noncognitivism "does not question the possibility of determining whether the adoption of such principles is rationally justified" (p. 28). By "such principles" Oppenheim means fundamental moral or evaluational principles. He further states that even with regard to important disagreements about "ultimate principles" such as the intrinsic desirability of racial discrimination, birth control, the death penalty, etc., noncognitivism need not deny the applicability of rationality (p. 171). In some sense, then, Oppenheim appears to suggest that the noncognitivist view is compatible with or perhaps requires rationality in deciding on basic moral principles. It is not clear what this amounts to, but Oppenheim makes several suggestions. He seems to suggest, at times, that being rational is partly a matter of choosing efficient means in attaining one's ends or goals. Empirical considerations as well as deductive reasoning are required (p. 148). Further, a rational agent attempts to foresee and predict the outcomes of all possible alternative courses of action and to determine whether his contemplated goal is compatible with other ends to which he has already committed himself (p. 171). Further, such "judgments of rationality" are either true or false (p. 33). His view seems to be summarized in the statement that "Judgments of rationality concern the adequacy of a choice of a course of action or policy in view of attaining a desired state of affairs, judged on the basis of information available to an actor in a given situation" (p. 28). On this view, then, instrumental value judgments are actually empirical statements; they in effect claim that a certain act is an effective means to attaining some further end. As such, their possible rationality is not questioned, and cognitivists and noncognitivists are not divided on this point.

My reason for quoting Oppenheim's remarks on rationality is that they give rise to a puzzle: The earlier quotation (the rationality thesis) suggested that the decision to adopt or not adopt "basic moral principles" is a matter, quite appropriately, up for rational assessment. Yet, given Oppenheim's elucidation of the concept of rationality, it is difficult to understand how this could be so. Normally Oppenheim clarifies his concept of rationality by emphasizing that a decision to act is rational if and only if it has certain characteristics, that is, it involves identifying alternative courses of action, foreseeing their respective outcomes, and reaching reliable estimates of the probabilities of their occurrence, and so on. A decision to act involves an instrumental value judgment, and ideally it is arrived at in such a way as to satisfy the above conditions. What is unclear is how Oppenheim connects this sort of rationality with the rationality of adopting basic moral principles. Surely one may be rational or not in choosing means to ends, admitting (as decision theorists and others have shown) that the criteria for predicating rationality are not as obvious or simple as one might think. Being rational, for Oppenheim, presupposes that the agent have certain ultimate ends. The tough question, as we know, is whether choices of ultimate ends can be said to be rational or not, or more or less so; if they can be, just what are the relevant criteria to be applied? One who chooses as an ultimate end the minimization of the wanton infliction of pain on human beings has, in effect, committed himself to a basic moral principle which might be modestly stated as: "I (the agent in question) ought not to wantonly inflict pain on human beings." Is this choice a rational one or not? Does it involve the adoption of a principle which, in Oppenheim's words, is "rationally justified"? Oppenheim appears to suggest that it might be, but given his view that such a principle is only an expression of feeling or attitude, it is not obvious how it could be. Since, by hypothesis, the adoption of such a principle is not a choice of means to some further end, the criteria he sketches concerning what it is to make a rational choice do not apply. His claim that noncognitivism does not question the possibility of determining whether the adoption of basic moral principles is rationally justified, is like a check which cannot be cashed in; at least he fails to dispel the feeling that his talk of rationality on this score is a delusion. The having of a feeling or an attitude may be *explained*; it is not obvious that it can be rationally *justified*. Indeed, a great deal of Oppenheim's discussion appears to defend just this point.

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[3] I deliberately oversimplify here. His characterization of noncognitivism also involves the suppositions (1) $N_1 \rightarrow N_2$, and (2) $N_2 \rightarrow N_3$. Correlative implications may be inferred but are not explicit in his characterization of cognitivism.

[4] One reason for dissatisfaction with Oppenheim's characterization of cognitivism is that the term "cognitivism" is often used in philosophical literature for the view that consists of C_2. One might well believe that moral judgments are true or false without believing that they can be known to be true or false in the sense that they can be *demonstrated* to be true or false. Analogously, one might believe in the existence of other minds or in the correctness of inductive inference without believing that there is any method of *demonstrating* the correctness of either belief. Further, one might have reason to believe that every even number is the sum of two prime numbers without believing that there is a known method of demonstrating the claim. Oppenheim tends to ignore this possible position when he states "If, on the other hand, either A or B is a cognitivist, he must be convinced that he can demonstrate the truth of his own basic ethical principle and the falsity of that of his opponent" (p. 34). In other words, while "cognitivism" and "noncognitivism" are mutually exclusive positions as Oppenheim defines them, they do not exhaust the interesting and competing positions being discussed in the literature.

As a matter of fact, there may be an important difference between feelings and attitudes, a point to which Oppenheim pays no attention. It may be that paradigm cases of expressions of feelings and attitudes are significantly different in that the former do not explicitly make claims which are true or false while expressions of attitudes do. For example, we typically say, "I feel hot" or "I feel run down." Neither of these utterances explicitly alleges that "It is hot" or "I am run down." For this reason the question of rational justification does not arise. On the other hand, we often say things like, "My attitude toward the reliability of Congressmen is that they cannot all be trusted." This last utterance would typically (though perhaps not explicitly) make a claim which is true or false; hence the question of rational justification would arise. Actually it must be admitted that the use of 'feel' or 'have the attitude that' and their cognate locutions is not a sure guide to whether or not a cognitive claim is being put forward. This is shown for example by the statement, "My feeling is that Ramsey Clark will be the next President." It would not be out of place to say, "Well, I think that is false." But this observation does not undermine my main point, which is that with at least some of the items we properly call "feelings" or "attitudes" the question of their truth or falsity—and hence the question of rational justification—does arise. Along these lines Oppenheim might have defended his remark about the possibility of a rational justification of basic moral principles. In this way one might attempt to reconcile the view that basic moral claims express attitudes or feelings *and* the view that their adoption is or is not rationally justifiable. To take this tack, however, would involve surrendering the notion that moral statements are neither true nor false, an essential ingredient in Oppenheim's noncognitivism. It would seem, then, that Oppenheim wants to have it both ways; I am not yet persuaded that this has been shown possible.

It might be objected that Oppenheim does not want to have it both ways, that he acknowledges that the choice of ultimate ends or commitment to ultimate moral principles is not rationally justifiable. There is strong support for this interpretation of his remarks on rationality. Indeed, it is the natural construal. He often appears to be arguing only in favor of the modest claim that a policy choice *relative* to some further goal may or may not be rational. For example, in regard to commitment to a moral principle, he states, "While such a commitment cannot be shown to be in itself legitimate or illegitimate, right or wrong, rational or irrational, it can have the support of preferences which, relative to this commitment, are rational" (p. 173). As we have noted, however, this construction leaves open the question of whether our most basic preferences or our fundamental moral commitments are rational or not. If a man has as his basic moral commitment the belief that he ought to maximize the wanton infliction of pain on human beings, then in Oppenheim's view his choices as to how to achieve this goal may be said to be rational or not. But it is not at all clear whether Oppenheim could say, with consistency, that the ultimate principle involved was itself rational or irrational. This point, of course, is the bitter pill, and it is one reason for avoiding a hasty approval of the noncognitivist view and avoiding the conclusion that "our ultimate moral principles have no other support than our arbitrary and hence blind preferences." Oppenheim denies that such a result is entailed by his claim that ". . . there are intrinsic valuational and moral issues which cannot be settled by rational argument . . ." (p. 173). But he has not shown us that the entailment does not hold. Whether it does or not requires further argument.

Oppenheim is aware of this sort of criticism, of course, and we have not yet fully canvassed his attempts to undermine it. He identifies the charge of perniciousness in the following way:

> However, noncognitivists cannot provide any guidance to ultimate standards. Hence it is argued, they are irrationalists as far as this most important question is concerned. If intrinsic value judgments are but expressions of subjective tastes, feelings, and preferences, then the most crucial and consequential moral decisions can only be arbitrary, capricious and irrational. And since all further decisions down the means-end chain depend on the choice of the ultimate end, all of them become arbitrary also, however rational they may be in terms of the final goal (p. 169).

In reply to the proposed difficulty, Oppenheim suggests that even if the charge were warranted it would apply only to a small number of choices, especially in the area of politics (p. 170). It is somewhat difficult to see how this admission helps remove the sting of the charge of irrationalism. Even if there were only one choice concerning an ultimate end (e.g., choosing to maximize the infliction of pain on human beings) that choice could certainly affect all subsidiary or intermediate-level policy decisions. Few would be optimistic about the prospect of falling under the influence of a man who was ideally rational about his choices of means to attaining such a goal; indeed most of us would prefer subjection to a stupid tyrant than to an intelligent, effective one. To say that noncognitivism concedes at most the irrationality of *only* a few choices provides a satisfaction on a par with knowing that one's physician possesses every skill of medicine with the qualification that he sometimes chooses to promote illness instead of health.

In further response to the charge of irrationalism Oppenheim points out that ultimate political goals are most often stated in terms so general that hardly anyone would deny being committed to them, e.g., "the general welfare," "equality of opportunity," or "the national interest" (p. 170). Now, it is obvious that the common approval of doing what is in the national interest is often only verbal agreement, the common use of a given slogan. What one political thinker means by "the national interest" is often radically different from what another means by it. What Mussolini meant by it does not coincide with what Walter Lippman has meant; such verbal agreement does not necessarily reflect any substantive agreement about normative political judgments. The fact that few would deny being committed to "democracy" or the "general welfare" may only reflect the virtually invariant characteristic of such terms to commend and to praise. Hitler as well was not averse to advocating "law and order," and appealing to the "will of the people" and "the national interest." Surely, we ought not to minimize the extent of substantive normative disagreement by calling attention to verbal similarities in various political theories or ideologies.

Even within the confines of a more or less homogeneous nation, assent to a common political creed is often quite misleading. For example, while few people in the United States would deny that they support or "believe in" the Constitution, there is considerable evidence that a very substantial proportion of United States citizens are in fact opposed to many of the civil liberties which the Constitution "guarantees" to American citizens. Oppenheim is quite aware of all this, but he nevertheless unduly plays down the depth and extent of disagreement about the "grand alternatives." One can, of course, say that intelligent, informed people need not take the ideologies of National Socialism or Fascism seriously. But this is just to beg the question. Why should they not be taken seriously? Why should they be rejected? Why should one choose a grand alternative such as equality of opportunity for all, as opposed to the view that there is a "hierarchy of nature" and that some deserve opportunities and powers that others do not?

Oppenheim clearly does not deny that the "grand alternatives" exist, even though he at points minimizes their importance. In fact, his preferences about certain fundamental options are clearly evident. In denying that noncognitivism entails nihilism, he states of his position that it

> . . . need not, and does not, prevent me from expressing my approval of democracy and denouncing tyranny. It is one thing to commit oneself to some moral principle; it is another to claim that it is demonstrable (p. 179).

Later, he urges that noncognitivism ". . . helps us to realize that it is not only impossible but also presumptuous to attempt to shape the world in our image" (p. 184). Further, "It helps us to hold the values of human dignity—fervently, but with humility" (p. 184). So, the general tenor of Oppenheim's normative commitments is clear enough. Still, the questions remain: Why should one oppose tyranny and approve of democracy? Why would it be wrong to attempt to shape the rest of the world in our image? Why should we uphold these alleged "values of human dignity"? And why should one prefer one policy or one goal rather than another? Why, indeed, does Oppenheim opt for one rather than another? On what grounds would he denounce tyranny?

It would seem that Oppenheim either can give reasons to support his choices among the grand alternatives or he cannot. If he could and did, he would be attempting to justify rationally his normative commitments. This attempt would involve the tacit supposition that normative judgments, the fundamental ones included, are the sorts of things that may be justified or not, reasonable or unreasonable, etc. But this supposition is, clearly, contrary to his noncognitivist premise that basic normative principles are neither justified nor unjustified, neither rational nor irrational, etc. The conclusion seems to follow that Oppenheim, on his own ground, cannot give reasons for certain basic and important aspects of his own normative preferences, e.g., his favoring democracy. But if so, it is not at all clear why he prefers democracy to tyranny. If he can only say "I prefer democracy" and he thinks it "presumptuous to shape the rest of the world" in his image, then should he not refrain from denouncing tyranny? After all, the tyrant can say, in a similar vein, "Well, I just prefer tyranny." I am glad that Mr. Oppenheim prefers democracy to tyranny, but what is mysterious is just why he does so. He appears to think that there is not just a psychological or sociological explanation of his preference, but that there are good reasons for preferring the one to the other, i.e., that his choice is not only an explainable but a justified one. However, his noncognitivist assumption implies, at the most *fundamental* level, that no reasons can be given and that his choice can be neither justified nor unjustified.

Since there is a high probability that my preceding remarks shall be misunderstood I wish to add a few qualifications. First, while I raise questions about whether a justification of Oppenheim's basic normative judgments can be given on his grounds, nothing in an *ad hominem* manner is intended. The general issue is simply whether a noncognitivist meta-ethic is consistent with, supports, or undermines certain basic sorts of normative judgments. Second, it does seem to me that arguments that a noncognitivist meta-ethic is innocuous with respect to basic moral judgments have not been convincing. There is disagreement on this ussue. For example, G. J. Warnock has claimed that while prescriptivism seemed to allow that moral debate is or can be a rational matter, ". . . this advantage turns out to be illusory: prescriptivism too cannot find much place for argument."[5] Further, if the *truth* of basic moral principles cannot be debated according to noncognitivism then there seems to be a clear sense in which those who think (for example) that the principle of utility is true are irrational. If a theory of political ethics rests on such a supposition (as did Bentham's, one might plausibly claim), then it would also be irrational in one key respect. So, it is difficult to believe Oppenheim's assertion that " . . every meta-ethical philosophy is logically compatible with every theory of political ethics" (pp. 181–182).

As we have noted, Oppenheim urges us to avoid the presumptuousness of attempting to shape the rest of the world in our own image, and sides with those who wish to uphold the values of human dignity. Further, he clearly thinks that the acceptance of the noncognitivist view somehow supports such attitudes and policies. He implies that there is some sort of natural alliance between the noncognitivist position and a normative commitment to toleration, human dignity, and humility about one's ideals. It is still not obvious to me why Oppenheim thinks that this is the case. On the contrary, it would seem that the "realization" that "there can be no objective foundation for our most basic moral and political convictions" would tend to undermine and weaken the moral convictions that one ought to be tolerant, that one ought not to impose his own views on the rest of the world, that one ought to uphold the values of human dignity. The belief that there is no rational way of deciding between certain fundamental and competing ultimate principles does not naturally or automatically lead to or support a tolerance of those who opt for quite different alternatives. One might opt for an ultimate principle which dictates intolerance and, while seeing no way to rationally defend such a view over others, *follow* the principle, i.e., try to shape the rest of the world in his own image. We should remember that religious believers have sometimes denied that their basic religious commitments are up for rational assessment at all, and at the same time have gone on to persecute those who hold different beliefs. I am not, of course, suggesting that the meta-ethical position of noncognitivism is morally pernicious; that would seriously beg the question. Rather my criticism here is an internal one. Oppenheim avers that there are no objective foundations for our basic moral convictions; presumably his own are included. If so, why does he commend a commitment to human dignity and toleration? Unless he has further argument to show otherwise, it appears that Oppenheim wants to perch on the liberal, humanistic limb of a tree whose trunk he is busily trying to cut down. Perhaps he thinks that there will be less moral presumptuousness when all the normative limbs strike the ground. But if and when the tree falls, not even Mr. Oppenheim's chosen branch will remain elevated.

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[5] G. J. Warnock, *Contemporary Moral Philosophy* (London: Macmillan and Company, 1967), p. 42.

Summary

In conclusion, I wish simply to review the major points for which I have argued. In the first part of this paper, I attempted to show that Oppenheim's rejection of cognitivism logically rests upon his defense of noncognitivism. His characterization of noncognitivism consisted of the three claims that ethical principles cannot be known to be true or false (N_3), that ethical principles are not true or false (N_2), and that ethical principles do not affirim or deny that something is the case (N_1). It is further evident that he regards N_3 as following from N_2, and N_2 as following from N_1. Thus his defense of noncognitivism rests, in a crucial way, upon his argument for N_1. Here I argued that the invocation of N_1 presupposes some workable criterion for determining just when the utterance of a sentence results in a statement which affirms or denies that something is the case and when it does not; such a criterion is essential to support N_1, but in fact Oppenheim fails to provide one. It is at this point that he asserts two claims: (1) that the function of ethical statements is to express attitudes or feelings, and (2) that if ethical statements perform that function then they cannot also affirm or deny that something is the case and hence are not true or false. I have questioned the claim that it is a unique and peculiar characteristic of all ethical statements to express attitudes or feelings (or to evoke them in others). I further argued that when a statement does perform such a function, it may also affirm or deny that something is the case and hence be true or false. Since Oppenheim makes little, if any, attempt to circumvent such difficulties, and since it is not clear how they *could* be circumvented, he has not therefore established his claim N_1 and hence has not shown us that noncognitivism is a correct view. It follows that he has not demonstrated the incorrectness of the cognitivist position, even conceding that cognitivism is confronted with serious difficulties. But if a reasonable man is one who proportions his belief to the evidence, then the reasonable man cannot conclude, on the basis of Oppenheim's arguments, that cognitivism has been shown false. Such has been my main contention.

Finally, I have also argued that Oppenheim has not rebutted the charge of irrationalism. He has, I have suggested, clearly opted for some of the grand alternatives as opposed to others. Yet we cannot tell why Oppenheim has the ultimate moral commitments he has in light of his claim that there are no objective foundations for our most basic moral and political convictions. It is not clear why he would denounce tyranny when, on his own view, the advocate of tyranny may have no better but no worse reasons for his conviction than does Oppenheim; indeed in Oppenheim's view it would seem that, at the stage of defending fundamental moral principles, neither can have any reasons at all. At the same time there appears to be an unreconstructed, unassimilated strain in Oppenheim's position for he sets forth what appear to be supporting reasons for accepting noncognitivism, some of which appear to presuppose that certain normative judgments are justified while others are not. As we have noted, he claims that noncognitivism curbs our tendency to be morally presumptuous, our tendency to act as if we were the moral guardians of the world, and that it helps us uphold values of human dignity. To invoke considerations which appear to be moral commitments themselves (e.g., one ought to be tolerant) or byproducts of such commitments is somewhat anomalous for one who argues that such principles have no objective foundations.

It is thus in the end unclear why Oppenheim, given his meta-ethical convictions, maintains the normative position he does. Further, he has not made a convincing case that there is some sort of natural alliance between noncognitivism and a liberal humanistic outlook. Yet he implies as much, and he does so in the face of his explicit claim that ". . . every meta-ethical philosophy is logically compatible with every theory of political ethics" (pp. 181–182). Last, I would like to reiterate that I have not attempted to show that noncognitivism is false or that cognitivism is correct; I have only sought to show that Oppenheim's defense has not provided us with a compelling argument for the correctness of noncognitivism. Perhaps Oppenheim and I would be equally hard put to provide a rational justification for our normative moral and political commitments; at any rate, so far as I can tell, on that score we are on the same side of the fence.[6]

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[6] While I have focused on what I take to be difficulties in Oppenheim's defense of noncognitivism, my objections should not be taken to imply more than they do. That is, Oppenheim has, I think, at numerous points successfully sorted out the relevant from the irrelevant charges that have often been made against both cognitivism and noncognitivism. Further he has provided a clear and useful account of how various classic and modern political thinkers are to be viewed from a noncognitivist standpoint. I know of no other account which possesses equal clarity and scope.

Those interested in examining the issues in further detail may wish to read Charles Stevenson's *Ethics and Language* (New Haven: Yale University Press, 1944), R. M. Hare's *The Language of Morals* (New York: Oxford University Press, 1964) and his *Freedom and Reason* (New York: Oxford University Press, 1965). Criticisms of noncognitivism may be found in Stephen Toulmin's *Reason in Ethics* (Cambridge: Cambridge University Press, 1950), Richard Brandt's *Ethical Theory* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1959) and G. J. Warnock's *Contemporary Moral Philosophy* (London: Macmillan and Company, 1967).

Comment: Defense of Noncognitivism Defended

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Professor VanDeVeer has given a scrupulously fair account of my defense of noncognitivism (N), and has raised questions which are both searching and stimulating.

1. His most basic criticism is that I fail to demonstrate N—the thesis that the utterance of a moral principle expresses the speaker's moral commitment, but does not affirm or deny that something is the case, and therefore does not result in a statement which is true or false. To provide adequate positive support for N would involve me—so he claims—in complicated problems of the theory of meaning. My reply is that I do, and can, rely on the very theory of meaning which, as Hempel states it, is generally accepted by contemporary philosophers of science, namely that

> . . . a sentence makes a cognitively significant assertion, and thus can be said to be either true or false, if and only if either (1) it is analytic or contradictory—in which case it is said to have purely logical meaning or significance—or else (2) if it is capable, at least potentially, of test by experiential evidence—in which case it can be said to have empirical meaning or significance.[1]

Given this basic principle (which Hempel says [p. 101] "is not peculiar to empiricism alone"), and given my demonstration that moral principles are neither analytic nor empirical, it follows that they are not cognitively either true or false.

2. I do indeed maintain, as VanDeVeer says, that the cognitivist arguments I discuss "exhaust the set of possible cognitivist attempts." Whoever holds certain moral principles to be cognitively true can do so only on *a priori* grounds, or else on the basis of empirical evidence. He must be either an intuitionist or a naturalist (in the broadest sense of these terms). I do not merely show that cognitivists have so far been unable to demonstrate their thesis—a point which VanDeVeer seems to concede. I cannot conceive of any future cognitivist advancing arguments which do not fall within one or the other category. If both intuitionism and naturalism are mistaken, cognitivism is mistaken.

3. And if cognitivism is false, N is true. VanDeVeer denies this inference, because he denies "that noncognitivism is the same as the denial of cognitivism" as I characterize these two positions. He claims that one is justified in believing that moral principles *are* true or false, without believing that they can be *known* to be true or false, just as one can "believe in the existence of other minds or in the correctness of inductive inference without believing that there is any method of *demonstrating* the correctness of either belief" (fn. 14). This comparison blurs an important difference. However puzzling the two latter questions are from a theoretical point of view, on a practical level we do not question the existence of other minds and the correctness of induction, because we have no choice but to behave as if these beliefs were true. The epistemological status of intrinsic value judgments, on the other hand, is an open question, and remains a matter of violent disagreement. Some cognitivists hold that doubting the truth of certain moral principles leads to unfortunate consequences; but this is surely no reason for believing them to be true. Suppose someone affirms: "such and such a moral principle is true; I don't know whether and how it can be demonstrated; but it is true just the same." We would hardly let it go at that. At the end, VanDeVeer concedes that he cannot establish the principles of political ethics to which *he* is committed any more than I can mine. Well, if "on that score we are on the same side of the fence," we must be on the same side all the way: the cognitivist claim in general is unwarranted; consequently N is the correct meta-ethical theory.

4. "There are utterances which involve true and false statements *and also* express feelings and attitudes." I agree, but disagree that this is an argument against N. While such utterances often contain words with both descriptive meaning and valuational connotation (e.g., 'promiscuous', 'snobs', 'racist' in the examples), it is always possible to separate analytically the statement made from the attitude expressed (e.g., your wife sleeps with other men; to do so is wrong). That the same speech act may have both "descriptive" and "dynamic" features is compatible with the view that the latter component lacks cognitive meaning.

5. VanDeVeer holds that, according to N, one can apply the criteria of rational decision making only to choices of means to given ends, but not to the adoption of ultimate goals or intrinsic moral principles. I claim that N is com-

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[1] Carl G. Hempel, *Aspects of Scientific Explanation* (New York: The Free Press, 1965), p. 101.

patible with the view that criteria of rationality can be brought to bear on the latter as well. For example, A's choice of x as an ultimate goal or principle may fail to be rational because x cannot be implemented by any means, or is incompatible with some other goal of A, or leads to side-effects which make the whole outcome of pursuing x less preferable *to A* than some alternative course of action which does not include x. This is precisely the reason why N does *not* entail that "our ultimate moral principles have no other support than our *arbitrary* and *blind* preferences." If A and B adopt different goals or principles without making such mistakes, however, I am indeed precluded from saying that A's choice or commitment is more, or less, rational than B's. To some, this may be a "bitter pill, and . . . one reason for avoiding a hasty approval of the noncognitivist view." But if, after unhurried reflection, this view turns out to be correct, we have no choice but to resign ourselves to the truth.

6. But am I not contradicting myself by affirming that two goals or moral principles are incompatible or compatible, conflicting or consistent? VanDeVeer says, "Ordinarily a set of sentences is said to be inconsistent just in the case where they cannot all be true at the same time and consistent where they can all be true together." Doesn't N deny that moral principles can be said to be true or false? True, according to N, two *sentences* like 'killing is wrong' and 'acting in self-defense is right' are neither compatible nor incompatible. Yet, the two corresponding *moral principles* are incompatible indeed. This may be construed as meaning that, were one to adopt both principles, there would be certain kinds of action—e.g., killing in self-defense—of which one would then both approve and disapprove. VanDeVeer himself admits the possibility "that 'consistency' can be cashed in without reference to truth or falsity, or affirmation or denial." Not only statements, but also attitudes, choices, commitments, and normative principles may be said to be compatible or conflicting.

7. Finally, my denial that my own moral principles of politics are true (or false) does not prevent me from *propounding* them and from trying to *persuade* others to adopt them. "Why does he commend a commitment to human dignity and toleration?" Because I am committed to these principles (old-fashioned as they may have become only a few years after the book was written—only temporarily, I hope). The connection I try to establish between the meta-ethics of N and the normative ethics of liberalism is one of psychological probability, not of logical necessity. Of course, there have been tolerant cognitivists and antiliberal noncognitivists. I merely claim that moral absolutism often leads to political absolutism, and that moral scepticism tends to promote pluralism. But this would be a poor argument for N. I believe that I have defended it on better grounds.

Rejoinder to Oppenheim's "Comment"

DONALD VANDEVEER
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I do not think that Professor Oppenheim and I are any nearer agreement, but perhaps the various forks in the road are clearer. I will try to be as fair, as clear, and to the point as Oppenheim has been. I will consider most, but not all, of his replies and will do so in the order he has followed.

Oppenheim concedes that he is relying upon the principle that a sentence is cognitively significant if and only if (briefly) it is logically significant or empirically testable, and he claims that this principle is generally accepted by contemporary philosophers of science. I do not think it is generally accepted; indeed, on the page after the one quoted by Oppenheim, Carl Hempel states that he feels "less confident" that such a criterion can establish "sharp dividing lines" between "those sentences which do have cognitive significance and those which do not."[1] The proper estimate of the current situation is, I think, that whether a general criterion of "cognitive" meaning can be had, and if so, just what it is—are notoriously unsettled questions. If any estimate is correct, it is that among philosophers of language there is widespread suspicion of the neat distinctions between analytic/synthetic, *a priori*/*a posteriori*, cognitively meaningful/meaningless which prevailed prior to and during the 1950's. The work of W. V. O. Quine and Noam Chomsky has only muddied the waters further. These remarks do not an argument make, but they call attention to the fact that contemporary investigations are liable to undermine rather than support Oppenheim's principle of meaning. To mention, finally, just one specific reason for doubting Oppenheim's principle, as stated it rules out (strictly speaking) as meaningless all questions and commands.[2]

In his third reply (3) Oppenheim attributes to me the view that one is justified in believing that moral principles *are* true or false without believing that they can be *known* to be true or false. But I did not make that claim; I claimed only that one might believe a moral judgment to be true or false without believing there was a way of demonstrating it to be so. One often has a good reason for thinking a claim to be true without knowing of an argument in favor of the claim that can plausibly be described as a "proof," or a "demonstration." But I left it open, whether or not the belief that the moral judgment is true was a *justified* one. I agree that the concession that no demonstration of a moral judgment is available does not let the moral judge off the hook; we do not "let it go at that." A request for justification of the judgment is appropriate; I did not claim or imply otherwise. Oppenheim invites a negative reaction to someone who affirms that a certain moral principle is true, that he does not know how it can be demonstrated but that "it is true just the same." The picture is of one who refuses to engage in *any* defense of his judgment. Such a man is made of straw. To the contrary, a man may (a) claim a moral judgment to be true, (b) concede he cannot demonstrate it, and (c) give an elaborate, reasoned defense of his judgment.

Oppenheim further states that I concede that I cannot establish the principles of political ethics to which I am committed any more than he can establish his. I did not concede quite so much; I only claimed that we might be equally hard put to do so. An important difference is that *I* think such a rational justification is possible; we are therefore not on the same side of the meta-ethical fence.

In regard to (4) it is plausible to read Oppenheim, at points, to be claiming (a) that moral utterances function so as to express feelings or attitudes, (b) that utterances which so function are not also true or false, and hence (c) that moral utterances are not true or false. I attacked (b) to show that this argument is unsound. Oppenheim apparently agrees that it is not sound and cannot be used to defend N_1; I welcome the agreement.

In regard to (5) I do not know that there is any readily agreed upon set of principles known as "the criteria of rational decision making." What I claimed, and there is some unclarity about this matter, is that Oppenheim's criteria (or at least some of them) do not apply to the adoption of ultimate goals since the criteria presuppose the existence of ultimate goals. At best, his criteria allow the rational assessment of some (possible) ultimate goals when others are already held and are not being called in question; this last claim is not implausible, but (a) it has not been argued in sufficient detail, and (b) it seems to be weaker than the sort of claim which Oppenheim hopes to establish.[3] In fact, Oppenheim's criteria of rationality do not seem to rule out the adoption of the goal of maximizing the infliction of pain on other human beings. Perhaps such a goal is not irrational, but I think there is a presumption that according to our ordinary concept of rationality (ill-defined as it may be), such a goal *is* irrational. If so, and if Oppenheim is trying to capture that part of our intuitive notion of rationality in his elucidation of the concept, then there is reason to think that he has not succeeded. Of course, to be bold, I agree that "we have no choice but to resign ourselves to the truth"; if, however, wildly different ultimate goals fail to violate any of Oppenheim's necessary conditions for being rationally chosen, and if his criteria allow no further discriminations, the question is: Have we gotten the proper explication of what it is to be rational? Further, can we arrive at it within the framework of noncognitivist assumptions?

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[1] Carl Hempel, *Aspects of Scientific Explanation* (New York: The Free Press, 1965), p. 102.

[2] This basic and widely discussed question concerning an adequate theory or criterion of meaning can be pursued in *The Theory of Meaning* edited by G. H. R. Parkinson and published by Oxford University Press.

In (6) Oppenheim tries to reconcile his claim that moral principles may be incompatible with his claim that moral statements neither affirm nor deny that something is the case and are neither true nor false. His proposed escape route is to concede that moral *sentences* are neither compatible nor incompatible, but corresponding moral *principles* may be, in the *sense* that subscribing to incompatible principles is tantamount to adopting conflicting attitudes, e.g., approving of and disapproving of the same sort of action. I find this unpersuasive. First, in his book Oppenheim makes no such distinction. Second, if one is going to try to have relations of incompatibility and compatibility without references to truth or falsity, as Oppenheim wishes to do with *principles,* why not allow the same for moral *sentences,* since this still avoids the result Oppenheim wishes to avoid—namely, attributing truth or falsity to moral sentences (or utterances, principles, etc.)? Third, in what sense are moral principles *incompatible* in his view? They must not be "logically incompatible," if by that expression we understand "cannot all be true," since this interpretation would not avoid the reference to truth and falsity. Oppenheim presumes some other sense of "incompatible." Turning to his example in (6), to say that the two corresponding moral principles are incompatible is to claim that if someone were to adopt them he would both approve and disapprove a certain sort of action. This is a highly problematic view for the following reason: It seems plausible to think that *no one can* (at the same time and in the same respect) both approve and disapprove a certain sort of action. I, for one, do not know how to make sense of the claim that Smith both approves and disapproves of killing in self-defense, for example. But if this view is correct, the allegedly equivalent claim that the two moral principles are incompatible would, likewise, seem to be senseless. That assertion however, is contrary to both common sense and Oppenheim; what must be faulty is, in my estimation, the proffered analysis of incompatibility. So, I conclude, we are still lacking an analysis of incompatibility, a concept Oppenheim uses in an essential way, which avoids reference to truth and falsity, a result Oppenheim needs to avoid.

In regard to (7) my concern was not primarily a psychological but a logical one. Oppenheim makes empirical claims concerning the connection between the various meta-ethics and various normative ethics. I would not quarrel with his weak claims, for example, that moral absolutism *often* leads to political absolutism. I gave reasons for doubting the strongly formulated ones, such as that moral scepticism *tends to* promote pluralism, that noncognitivism "helps us" to realize the impossibility and presumptuousness of trying to shape the world in our image, that it "helps us" to uphold the values of human dignity. But these are empirical issues, and a philosopher would be presumptuous to suggest that the defense of such empirical claims requires empirical evidence which we do not appear to have.

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[3] Compare his remark that "Judgments of rationality concern the adequacy of a choice of a course of action or policy in view of attaining a desired state of affairs. . . ." Felix Oppenheim, *Moral Principles in Political Philosophy,* p. 28.

Freedom of Information and the Evidentiary Use of Film in Japan: Law and Sociopolitics in an East Asian Democracy*

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Although the question of a "newsman's privilege" concerning confidential news sources has been debated in Japan as elsewhere, this issue has been minor compared to what may be termed the "media privilege" problem.[1] On August 28, 1969 a Fukuoka District Court (Kyūshū) ordered four local television stations to present for use as evidence film taken during a student-police encounter at Hakata Station (Fukuoka) on January 18, 1968. The media refused to comply, even after the Supreme Court struck down their appeal against the order in a unanimous decision. Portions of the film were finally seized on March 4, 1970.

The information media maintain that Article 21 of the Constitution of Japan guarantees them the right to determine, in any given case, whether or not still photographs and cinematic film may be used as court evidence.[2] This claim extends to already published material and to news sources with whom no confidential relationship exists. Although the media have not been consistently uncooperative with the courts over the years, the 1960's saw an increase of controversies involving the prerogatives of courts, prosecutors, and mass media, and a major confrontation developed with the Hakata Station film case in 1969. This study sets forth the status of Japanese constitutional and procedural law concerning court-media relations, while pointing up some aspects of the interaction of law and political culture in Japan, such as the manner and degree to which open confrontations are studiously avoided.

I. The Mass Media and Law in Japan

Before examining the Hakata case in detail and surveying related cases, we should note some factors affecting the framework of controversy. Japan is blessed with a free and vigorous press which provides thorough and generally responsible coverage of both national and international events.[3] The diffusion of news

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[1] The term "media privilege" is used herein to capsulize the right claimed by Japan's mass media regarding film and photograph evidence. The Japanese words "*hōdō no jiyū*" (freedom of information) denote free access to information on the part of both gatherers and consumers of news, as well as freedom to report news.

Concerning the legal status of the "newsman's privilege" in the United States, see "The Newsman's Privilege," a subcommittee report to the Committee on the Judiciary, U.S. Senate, October, 1966; a Note, "Privileged Communications-Mass Media—A 'Shield Statute' for Oregon?" 46 *Oregon L. Rev.* 99 (1966); J. E. Beaver, "The Newsman's Code, the Claim of Privilege and Everyman's Right to Evidence," 47 *Oregon L. Rev.* 243 (1968); J. A. Guest and A. L. Stanzler, "The Constitutional Argument for Newsmen Concealing Their Sources," 64 *Nw. U. L. Rev.* 16 (1969); Talbot D'Alemberte, "Journalists Under the Axe: Protection of Confidential Sources of Information," 6 *Harv J. Legis.* 307 (1969).

None of the "newsman's privilege" disputes which arose in the U.S. during 1970 seems to have involved a claim of privilege extending to already telecast or published material, or to news sources with whom no confidential relationship existed. Such claims were made in Japan.

[2] Article 21 of the Constitution of Japan:
"Freedom of assembly and association as well as speech, press and all other forms of expression are guaranteed.
2. No censorship shall be maintained, nor shall the secrecy of any means of communication be violated."
The Constitution of Japan, Supreme Court, Tokyo, 1968.

[3] Only a brief clarification of this statement can be offered here. The information media are quite free of overt government restraints in gathering and reporting news. The degree to which freedom of information is limited by self-imposed industry restraints and informal official persuasion is debated, as is the extent to which the press supports or opposes government policies in Japan's public dialogue.

The editorial views of the leading news media on important political issues do not often suggest marked differences of perspective within the information industry. Nor do they consistently represent either majority opinion—as reflected in opinion polls or elections results—or official policies or the opinions of a particular opposition political party. (A useful compilation of Japanese opinion poll data is provided by the *Gekkan Seron Chōsa* [Opinion Research Monthly], Finance Ministry Printing Office, Tokyo.) On balance, the media seems establishmentarian, quite through national and local newspapers and television is unusually complete.

The combined daily circulation of Japan's newspapers is more than 51 million copies, of which almost 10 million copies go to readers in Tokyo. The "three giant" national newspaper companies—*Asahi Shinbun* (circulation, 9.3 million), *Yomiuri Shinbun* (8.4 million), and *Mainichi shinbun* (7.3 million)—also publish a number of mass circulation magazines, books and small English-language dailies, and they operate national broadcasting networks. More important than any of these commercial networks or local radio and television companies is publicly owned NHK (Nippon Hōsō Kyōkai [Japan Broadcasting Corporation]), whose radio and television programming reaches into virtually every Japanese home.

Most newspapers (111 or 118), many broadcasting companies (45 of 95), the news agencies (8), and NHK belong to the *Nihon Shinbun Kyōkai* (Japan Newspaper Publishers and Editors Association; hereafter referred to as Shinbun Kyokai). Forty-six commercial broadcasting companies, many of which belong to the Shinbun Kyokai as well, are members of NAB (National Association of Commercial Broadcasters [*Nihon Minkan Hōsō Renmei*, or commonly *Minpōren*]).[4]

These organizations do not set policies for individual firms or local groups of media companies, and the different components of the "three giant" empires operate with considerable editorial independence. On the other hand, the media operate under a common industry code,[5] and the media organizations, after con-

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nationalistic, and liberal rather than radical or ultra-conservative.

A world survey of press freedom rates Japan rather highly, but cites industry centralization and self-regulation as problems, "World Press Freedom, 1966," Freedom of Information Center Report, No. 181, School of Journalism, Univ. of Missouri, May, 1967. A Japanese specialist, commenting on the report, mentions interference with freedom by political groups and ultra-rightists as an additional problem. Hideo Shimizu, *Hō to Masu Komyūnikēshon* (Law and Mass Communications) (Shakaishisosha, 1970), 75–79. All Japanese literature cited in this article was published in Tokyo.

Reporters from competing newspapers are organized into "press clubs" (*kisha kurabu*), each of which is attached to particular politicians or a government agency. Rather than competing with each other for news, members of a press club form a coherent group which determines what news is suitable for release. At times, stable personal ties develop between a press club and its news sources. In such cases, the press club is usually kept well informed, but with the understanding on all sides that some information may not be meant for immediate public consumption. One who would deviate from the group's consensus in reporting news invites severe ostracism. Newspaper editors generally swim with the current. This mode of operation (which should be kept in mind when assessing the media's stress on the people's right to know) arises quite naturally in Japan's elitist, group-oriented, and consensus-conscious society. It does not imply that the mass media lack a strong sense of independence from government when they (always in company with other elites) feel criticism of policy is called for.

For critical views of the media, see Takuro Namino, *Shirarezaru Hōsō* (The Unpublicized World of Broadcasting), Gendai Shobo, 1967; Nihon Janarisuto Kaigi, ed., *Masu Komi Kokusho* (Mass Communications Black Paper), Rodojunposha, 1968. In English, see Edward P. Whittemore, *The Press in Japan Today: A Case Study* (Columbia: University of South Carolina Press, 1961); George R. Packard, *Protest in Tokyo: The Security Treaty Crisis of 1960* (Princeton: Princeton University Press, 1966), pp. 278–284 and *passim;* Richard Halloran, *Japan: Images and Realities,* (New York: Alfred A. Knopf, 1969), pp. 159–184. All these American writers refer to instances of nationalistic, emotion-charged, anti-American press campaigns. An occasional nationalistic "mood" (*mūdo*) does sweep through Japan's media; but day in and day out, the mass media provide very substantial coverage of the U.S., most of it fairly accurate, very little of it openly hostile. American press coverage of Japan seems minimal and of mixed quality. Limited consumer receptivity may be much less important a reason than the Europocentrism and shallow interest of publishers and editors, and the "*kanji* (ideograph) curtain" (i.e., linguistic and sociocultural barriers). The major non-Western power and America's most important single partner deserves more and better coverage.

[4] Also noteworthy are the very influential *Nihon Keizai Shinbun* (Japan Economic Journal, 1.8 million); *Chūnichi Shinbun* (3 million); and *Sankei Shinbun* (3 million). NHK operates two TV channels (over 1,500 stations) and three radio networks (480 outlets).

See *The Japanese Press,* published annually by the Shinbun Kyokai since 1949; the more substantial *Nihon Shinbun Nenkan* (Japan Newspaper Yearbook), Shinbunkyokai; and *Shuppan Nenkan* (The Publishers Yearbook), Shuppan Nyūsusha.

[5] "The Canons of Journalism," *The Japanese Press* (1970), p. 4. See also the annual *NHK Handbook* in English "Hōso Hō" (The Law of Broadcasting), "Shuppan Hō" (The Publication Law), and "Shinbun Hō," (The Newspaper Law), in *Roppo Zensho,* Yuhikaku, 1970; M. Ito and H. Shimizu, eds., *Masu Komi Hōrei Yōran* (Survey of Mass Media Regulations), Gendai Janarizumu Shuppankai, 1966; and "Masukomi Hanrei Hyakusen" (One Hundred Leading Cases Concerning Mass Communications), *Jurisuto,* extra edition No. 31, Feb., 1971.

Concerning the development and regulation of the mass media in Japan, see these Shinbun Kyokai publications: *Nihon Shinbun Kyōkai Nijūnenshi* (The Twenty-Year History of Shinbun Kyokai), 1967; *Masu Komi no Shakaiteki Sekinin* (The Social Responsibilities of the Mass Media), 1966; and *Shinbun to Hōritsu* (Newspapers and the Law), 1960. See also, NHK's substantial *The History of Broadcasting in Japan,* 1967 (not for sale): John A. Lent, ed., *The Asian Newspapers' Reluctant Revolution* (Ames: Iowa State University Press, 1971), pp. 65–87; the *Bulletins* of Shinbun Kenkyūjo (Institute of Journalism), Tokyo University; Masami Ito, *Genron-Shuppan No Jiyū* (Freedom of Speech and Press Freedom), Iwanami Shoten, 1959; Shimizu, *op. cit.;* Yasuhiro Okudaira, sultation with company representatives, do issue policy statements (as in the Hakata case) which carry considerable weight throughout the mass communications system. This rather oligopolistic and well integrated media system, made even more cohesive by complex personalities among industry elites in such major cities as Tokyo and Osaka, facilitates strong national response to challenges such as the Hakata film controversy.

Another important factor conditioning court and mass media perceptions of the Hakata case is the nature of Japan's legal and judicial systems.[6] Although the Constitution of Japan speaks of the Diet as "the highest organ of state power" (Article 41), it also empowers the Supreme Court "to determine the constitutionality of any law, order, regulation, or official act" (Article 81). All laws are national laws, and all courts come under the administrative and rule-making authority of the Supreme Court. Under Japan's "three-stage trial" system (i.e., usually the district, high, and Supreme Courts), a prosecutor may appeal against an acquittal or a lenient penalty. While Japan is basically a code law country (following continental models), the Anglo-American common law approach has steadily increased in importance there since 1945. Both Western systems have undergone notable transformations, in application if not always in the letter of the law, as they have been absorbed into Japan's sociolegal system; the foreign student of Japanese law and politics does not easily maintain balanced perspective on the relative importance of different ingredients in the mix.

In developing doctrines on issues not yet settled by the Supreme Court and in refining established interpretations of law, judges at all levels take note of precedents and, to a lesser extent, a voluminous body of scholarly commentaries which range from rather abstract theoretical pieces to meticulous case analyses. At times, the press and many professors of constitutional law, whose views the mass media frequently solicit,[7] have been rather critical of the Supreme Court and some lower courts for excessive caution in their civil liberties decisions. Controversial decisions of the more liberal district court judges sometimes occupy a more important place in political discourse than in law, since they are often overturned by subsequent high court or Supreme Court decisions.[8] More im-

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Hyōgen no Jiyū—to wa Nanika (What Is Freedom of Expression?), Chuo Koronsha, 1970; Fumio Yamamoto, *Nihon Masu Komyūnikēshonshi* (The History of the Mass Media in Japan), Tokai Daigaku Shuppankai, 1970; Ryukichi Kitagawa *et al*, eds., *Gendai no Masu Komyūnikēshon* (Mass Communication Fact Book), Aoki Shoten, 1970; Taro Tadokoro, *Shuppan No Senkusha* (Pioneers in the Publishing Field), Kobunsha, 1969; Rokuro Hidaka *et al*, eds., *Sengo Shiryo: Masu Komi* (Postwar Materials: Mass Communications), Nihon Hyoronsha, 1970.

[6] In English, see Japanese-American Society for Legal Studies, *Law in Japan: An Annual*, Tokyo, University of Tokyo Press, from 1967; Arthur T. Von Mehren, ed., *Law In Japan* (Cambridge: Harvard University Press, 1963); Dan F. Henderson, ed., *The Constitution of Japan: Its First Twenty Years, 1947–1967*, (Seattle: University of Washington Press, 1969); the Introduction to John M. Maki, *Court and Constitution in Japan: Selected Supreme Court Decisions* (Seattle: University of Washington Press, 1964); Shigemitsu Dando, *The Japanese Law of Criminal Procedure*, trans. B. J. George (South Hackensack, N.J.: Fred B. Rothman and Co., 1965); Glendon Schubert and David J. Danelski, eds., *Comparative Judicial Behavior* (New York: Oxford University Press, 1970), *passim;* and the Supreme Court's English-language publications, *Outline of Japanese Judicial System* (1968) and *Outline of Criminal Justice in Japan* (1969). Very little scholarly writing on contemporary Japanese Law, even of article length, has appeared in any other Western Language. The notable exception is Yosiyuki Noda's *Introduction au Droit Japonais* (Paris: Librairie Dalloz, 1966). However, a few young European scholars are now in training.

[7] For example, in connection with the film evidence controversy, the sympathetic views of Dean Masami Ito (Faculty of Law, Tokyo University) frequently appeared in the mass media. See *Asahi Shinbun* (The Asahi Newspaper; hereinafter cited as *AS*), Sept. 9, 22, Oct. 21, 26, 1969. On the same issue, the views of another eminent constitutional lawyer, sought on the mistaken assumption that he agreed with the media's position, were never published. The author was with Dean Ito when, barraged by the media for his reactions within minutes after the Hakata film seizure, he counseled calm and compliance, in view of the Supreme Court's decision. (See Section VI of this paper.) Published or broadcasted *zadankai* (round-table discussions) concerning public issues have become an important Japanese institution since they began in the 1920's, and are one of the ways professors, especially Tokyo University professors, exercise a continual influence on Japanese sociopolitical life.

There is no real parallel in the U.S. to the pervasive social, political and economic influence of Tokyo University, particularly its Faculty of Law. See Akira Kubota, *Higher Civil Servants in Postwar Japan* (Princeton: Princeton University Press, 1969), pp. 68–71; Hideo Shimizu, *Tōkyō Daigaku Hōgakubu: Nihon Erīto no Manmosu Kichi* (The Tokyo University Law Faculty: The Mammoth Base of Japan's Elite), Kodansha, 1965. Japan's official case reporting system moves slowly and does not publish all decisions. On the other hand, Japan's national newspapers provide detailed reports on the background and contents of important court decisions on decision day (usually Wednesday), and the full texts of many major court opinions appear within a month or so in such journals as *Hanrei Jihō* (Nihon Hyoronsha).

[8] Note, for example, the Takada case (note 16 below) and the Sapporo high court's quashing of a district court decision on the highly political Naganuma Nike Missile Site Case, *AS*, Jan. 23, 1970; and *Hanrei Jihō* (No. 565) 23 (Sept. 21, 1969); Lawrence W. Beer, "The Public Welfare Standard and Freedom of Expression in Japan," in Henderson, *op. cit.*, p. 205.

portant than scholarly, political, and judicial debates about freedom, the Japanese enjoy and value freedom of expression.

Among democracies, peoples differ in the styles and degrees of freedom of expression they prefer. The unwritten rules of society, rather than formal law or political power, seem to define the limits of freedom in Japan. In behavioral terms, ingroup constraints on individual freedom seem to weigh much more heavily than governmental restraints in the balance which the Japanese strike between freedom and social discipline; and a Japanese generally seeks self-identity and accomplishment more naturally as a member of a group than as an individual facing the world alone.

On a per capita basis, demonstrations are probably more frequent in Japan than in any other democracy in the world. If a day passed without a demonstration in Tokyo, that fact could be treated appropriately as headline news. During the past 25 years, trials and judicial decisions arising from such political activities have often focused national attention on the courts. Since political groups commonly invoke defense of the Article 9 "no war" clause or some human rights provision of the Constitution (Articles 10 to 40) as the underlying reason for demonstrations of petition or protest, Japan's Constitution plays a peculiarly important role in political rhetoric. Although some constitutional lawyers and political analysts have consistently eschewed rigidly ideological thinking, the Constitution, various shades of Marxism, and specific issues are commonly integrated in the public polemics of social critics, scholars, socialist politicians, union leaders and student activists. (Extreme rightists are fewer and, like members of the Japan Communist Party, are generally less open about their activities than other political groups.) Thus, demonstrations concerning a teacher-rating system, the U.S.-Japan Security Treaty, university reform, the Self-Defense Force, property rights, or pollution control become enmeshed in public discussion of the Constitution, ideology, law, and judicial actions affecting the rights of demonstrators.[9]

Indeed, the problems of news coverage and the gathering of evidence about public disturbances have formed the focal point of the media privilege controversy. It is usually the prosecution (rather than the defense or the bench) which takes the initiative in acquiring photographs or film from the mass media for presentation in courts. The film evidence issue has more often occasioned tensions between the mass media and prosecutors or police than between the media and the courts. In the Kokugakuin film dispute,[10] the respective prerogatives of investigating officers and the courts were at issue. The cases surveyed below clarify the modalities of court-media relations in post-1947 Japan prior to the Hakata Station film controversy.

II. The "Newsman's Privilege"

The first and only Supreme Court decision to date involving a reporter's refusal to divulge a news source on grounds of a newsman's privilege was handed down by the Grand Bench on August 6, 1952. In late April, 1949, Kiyoshi Ishii, an *Asahi Shinbun* reporter in Matsumoto City, wrote about the impending arrest of a local tax official before the matter was made public by police. During the subsequent trial of the tax official and investigation of the illegal information leak, Ishii refused to divulge to the Nagano district court the source within officialdom of his early knowledge of the affair. According to the court, this knowledge extended to detailed information in the arrest warrant itself. His claim of "privileged communication"

The semi-monthly *Jurisuto* (Yuhikaku) and the monthly *Hōritsu Jihō* (Nihon Hyoronsha) are the most influential of dozens of journals concerned with law and politics, with the former having an edge among jurists. In regular or special issues, these two journals often provide the most comprehensive analyses available of major sociopolitical and legal issues. Also quite influential are: the monthly *Hōsō Jihō,* (Hōsōkai) which contains official expatiations on court doctrine; specialized journals such as *Amerika Hō,* the sister publication of *Law In Japan: An Annual* (See note 6) and *Kōhō Kenkyū* (Yuhikaku), the organ of the Japan Public Law Association; and very general, elite-oriented journals such as *Chūō Kōron* (The Central Review).

Japan's "functional equivalent" to American political science may be a hybrid of Japanese political science (minus some of its historical stress) and the more highly developed interdisciplinary field of "constitutional science" (*kempōgaku*). Unfortunately, the work of most of Japan's leading scholars is known to very few foreign political scientists. For background on constitutional science, see Frank O. Miller, *Minobe Tatsukichi: Interpreter of Japanese Constitutionalism* (Berkeley: University of California Press, 1965).

[9] Concerning group behavior and demonstrations, see note 69 below and accompanying text, and Lawrence W. Beer, "Japan 1969: 'My Homeism' and Political Struggle," *Asian Survey,* 10 (January, 1970), pp. 43–47; in Japanese, see "Chian to Jinken" (Public Peace and Human Rights), a special issue of *Hōritsu Jihō,* No. 498, June, 1970. For the prewar modern history of courts and politics, see Sakae Wagatsuma *et al,* eds., *Nihon Seijisaibanshiroku* (Political Trials in Japan's History), 5 Vols., Daiichi Hoki, 1968–1970.

[10] The Kokugakuin University film seizure case is discussed in Section IV of this paper.

was strongly supported by both his employers and the Shinbun Kyokai.

The Supreme Court unanimously quashed his appeal from a conviction for refusing to be sworn in to testify, and Ishii was fined 3,000 yen (about $8.50).[11] The court noted that newsmen's communications are not accorded privileged status by the Code of Criminal Procedure,[12] that the establishment of such a privilege is a legislative matter, and that Article 21, which guarantees freedom of expression evenhandedly to all, is not violated by the court's requirement that the newsman reveal his sources. If newsmen could appeal directly to Article 21, the courts said, writers other than reporters would be in a position to claim privilege, thus obstructing criminal justice and allowing discriminatory application of Article 21 to the benefit of writers.

Ishii's fine was paid in 1952, and no further action was taken, since charges against the tax official had been dropped in the meantime. Then, as later, efforts by newspaper interests to have a newsman's privilege established by law proved ineffectual. From that time to this writing, confrontations between the court and newspaper companies such as occurred in the Ishii case have been avoided, and no newsman has been brought to the bar for refusing to testify.[13] The major issue in Japanese constitutional and procedural law has been the more broadly conceived "media privilege" regarding evidentiary use of film and photos.

III. The Use of Media Photographs as Court Evidence

Newspaper and magazine photographs have been used as court evidence when police photographers have not been at the scene of a disturbance, when picture-taking by police officers has been obstructed, or when other third-party evidence was not forthcoming and evidence in hand was not deemed adequate by the prosecution or the court. The methods used by police and prosecutors to acquire media photographs have varied, but they generally accord with the principle of voluntary presentation.

When a newspaper or magazine does not voluntarily submit the photographs requested in discussion, a search and seizure warrant (not a *subpoena*) may be secured from a court and discussions with the company continued.[14] As a rule, some or all of the desired photos are ultimately handed over without resort to coercion and search of company premises. As far as possible, prosecutors ask the media only for copies of photographs which have already been published. Occasionally, when company management has withheld photographs from officials, someone within the firm's offices has cooperated by anonymously presenting the desired items. The following brief description of the major cases of photograph use as evidence from 1945 to 1970 will provide a clearer idea of variations in official, activist, and mass media patterns of behavior.

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[11] 6 *Keishū* (No. 8) 974 (1952). For a translation of the Ishii decision, see Maki, *op. cit.*, p. 38. Ishii was convicted under Article 161 of the Code of Criminal Procedure (*Criminal Statutes*, I, Ministry of Justice, Tokyo, 1961, p. 97):

> "Any person who refuses to be sworn or to testify without due reason shall be punished with a fine not exceeding 5,000 yen or penal detention.
> 2. In the case mentioned in the preceding paragraph, both fine and penal detention may be imposed according to circumstances."

Due to postwar inflation, a 1948 law raised the maximum fine by a factor of fifty, *Roppō Zensho*, Yuhikaku, 1970, p. 1689.

[12] Regarding privileged evidence and communication, the Code of Criminal Procedure provides:

> "Article 105. A person who is, or was, a doctor dentist, midwife, nurse, practicing attorney, patent agent, notary public or a religious functionary may refuse seizure of articles held in his custody or possession in consequence of a mandate he has received in professional lines and which relates to secrets of other persons. However, this shall not apply if the client has consented to such seizure, or if the refusal of seizure is deemed as nothing but an abuse of right intended merely for the interest of the accused when he is not the client or if there exist any special circumstances which shall be provided by the Rules of Court" (*Criminal Statutes*, p. 86).
>
> "Article 149. A person who is, or was, a doctor, dentist, midwife, nurse, practicing attorney, patent agent, notary public or a religious functionary may refuse testimony in respect to facts of which he has obtained knowledge in consequence of a mandate he has received in professional lines and which relate to secrets of other persons. However, this shall not apply if the client has consented, or if the refusal of testimony is deemed as nothing but an abuse of the right intended merely for the interest of the accused when he is not the principal or if there exist any special circumstances which shall be provided by the Rules of Court" (*Criminal Statutes*, p. 94).

[13] *AS*, Sept. 9, 1969. However, a journalist has, for example, unsuccessfully attempted to escape liability for defamation on grounds that the public nature of his profession absolved him from the need to testify in court in order to prove the truth of his allegations, Supreme Court, Dec. 7, 1955, 9 *Keishū* (No. 13) 1633 (1955).

[14] Concerning seizure warrants and *subpoenas*, see notes 31 and 44 below. For a discussion of the still photo cases, see Hiroshi Ishikawa, "Hōdōshashin-Terebifirumu ni yoru Risshō no Dōkō" (Trends and Issues in the Evidentiary Use of Media Photographs and Television Film), *Jurisuto* (No. 439) 43 (Dec. 1, 1969). This issue of *Jurisuto* is hereafter cited as *Jurisuto*, No. 439. See also Zenji Ishimura, "Firumu Teishutsu Meirei to Hyōgen no Jiyū" (Freedom of Expression and the Order to Present Film), *Hōritsu Jihō* (No. 489) 78 (November, 1969), and in the same issue, Kiyoshi Fukuoka, "Hakata kara no Repōto" (Report from Hakata), at p. 90; Yasuhiro Okudaira, "Saikōsai to Hōdō no Jiyū (The Supreme Court and Freedom of Information), *Hōgaku Seminah* (No. 169) 7 (March, 1970); Michio Inaba, "Hōdō no Jiyū towa Nanika" (What Is Freedom of Information?), 2 *Gendai no me* (No. 6) 84 (June 1, 1970); Lawrence W. Beer, "Nihon ni okeru Masu Komi to Kyōgen no Jiyū" (Mass Communication and Freedom of Expression in Japan), *Amerika Hō* (2–1970) 173 (1971). The principal locus in the Code of Criminal Procedure concerning search and seizure is Articles 99 and following, *Criminal Statutes*, p. 84.

In the unsettled final years of the Occupation before the Japan Communist Party changed to a so-called "lovable image," incidents of communist violence were not rare, and leftist influence in unions sometimes made management hesitant to cooperate with the law. Following the Taira disturbance (Fukushima Prefecture) of June, 1949, most of the suspects were identified with the help of 87 newspaper photographs. Political activists had occupied the Taira City Hall, placed a red flag at the entrance, and allowed a newsman to take pictures in memory of the occasion. In an attendant scuffle, police film was seized and exposed. The district prosecutor requested and received the newspaper photographs, but obtained a *pro forma* seizure warrant to avoid exacerbating the newspaper's relations with the Communist Party.[15]

Another form of prosecutor-newspaper compromise occurred after the Osu disturbance of 1952, on which a judicial decision was not forthcoming until late 1969.[16] The Nagoya district prosecutor sought and received photographs from five newspaper firms (*Asahi, Mainichi, Yomiuri, Chūbu Nihon, Nagoya Taimuzu*), on the understanding that newsmen would not be asked to testify and that the precise source of any given photograph would not be divulged. The prosecutor, lacking any police photos of usable quality, presented in court a booklet of 27 anonymous photographs, of which 20 helped substantially in identifying participants in the disturbance. Another instance of news media cooperation occurred following a clash between pickets and police in Miyagi Prefecture in September, 1955 (the Ōtakane incident). Photographs presented voluntarily and without a *pro forma* warrant by the *Kahoku Shimpō* newspaper of Sendai proved helpful in obtaining convictions.[17]

In sharp contrast to the cooperation evidenced above was the Ōji photograph case. In September, 1958, violence broke out between two rival unions at the Ōji Paper Mill on Hokkaido. *Hokkaidō Shinbun* (The Hokkaido News) cameramen were more successful than the police in taking photographs usable as evidence. Since the newspaper refused to comply with official requests, the Sapporo district prosecutor, armed with a warrant, confiscated five newspaper photos from a department store where they had earlier been on post-publication display. Local newspapers and unions and the Shinbun Kyokai protested the precedent-setting seizure, contending as others would in later years that parties to future disturbances would be less cooperative with cameramen if they thought pictures might be used as evidence against them.[18]

Among the most consistently activist organizations in Japan is *Nikkyōso*, the Japan Teachers' Union. Teachers' union opposition to scholastic achievement tests at Nagayama Middle School (Asahikawa City, Hokkaido) in 1961 resulted in an unceremonious confrontation with the principal in the school halls. Subsequently, evidentiary photographs were clipped from the local newspaper.[19] In 1959 after some teachers' union members were indicted for manhandling the Oita prefectural superintendent, four photographic negatives mailed to the prosecutor's office by an unidentified party were used as evidence in court. Various circumstances suggested the negatives came from a newspaper office, but the local newspapers did not cooperate, at least publicly, and full details were never known.[20]

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[15] Ishikawa, *op. cit.*, pp. 44, 45. Involved in this and a number of later cases was alleged violation of The Penal Code, Chapter V: Crimes of Obstruction of the Performance of Official Duties, *Criminal Statutes*, I, p. 21:

> "Article 95. (Obstructing or Compelling Performance of Official Duty)
> A person who uses violence or intimidation against a public officer engaged in the performance of his duties shall be punished with imprisonment at or without forced labor for not more than three years.
> 2. The same applies to a person who uses violence or intimidation against a public officer in order to cause him to perform or refrain from performing an official act or in order to bring about his resignation."

Supreme Court, First Petty Bench, December 8, 1960; 14 *Keishū* 1818 (1960), convicted of the "crime of riot" (*s̄oranzai*); Robert A. Scalapino, *The Japanese Communist Movement, 1920-1966* (Berkeley: University of California Press, 1967), provides background information.

[16] See *AS*, Nov. 10, 11, 12, 1969, and the editorial, *Asahi Evening News*, November 12, 1969, concerning the Osu Case convictions. On September 19, 1969 another Nagoya district court had acquitted defendants in the somewhat analogous Takada Case on grounds of unconstitutionally delayed justice; but the Nagoya high court reversed this holding, *AS*, Sept. 19, 1969, July 16, 20, 1970. The Taira Case and the Osu, Suita, and May Day incidents of 1952 are collectively referred to as the "Four Great Public Disturbance Cases." All these cases involved delayed justice, in part because of the large number of defendants on trial and in part due to their political sensitivity even years after the events. The first judgment on the 1952 May Day case was handed down on Jan. 28, 1970, *AS* (evening ed.), Jan. 28, 1970, and "Mēdē Jiken" (The May Day Case) a special issue of *Jurisuto*, No. 446, March 15, 1970; in English, see Lawrence W. Beer, "Japan Turning the Corner," *Asian Survey* 11 (Jan., 1971), p. 78.

[17] Ishikawa, *op. cit.*, p. 44; Supreme Court, First Petty Bench, June 23, 1966, 159 *Saikōsaibanshū* 1229 (1966), conviction.

[18] Ishikawa, *op. cit.*, p. 44; *AS*, Sept. 9, 1969; Supreme Court, First Petty Bench, July 8, 1965 (unreported case), appeal against conviction dismissed

Similarly, an anonymous person left on the desk of an absent police official an envelope containing negatives which showed an illegal distribution of election propaganda outside a Shimonoseki City Hall courtroom (April, 1963).[21]

More recently, at the request of defense counsel, the Tokyo district court asked for the negatives of somewhat blurred pictures of the "First Haneda Incident" which had appeared in *Shūkan Asahi* (*Asahi Weekly*, November 3, 1967). These photographs showed students clashing with police as Prime Minister Sato left on a controversial trip abroad. Appealing to freedom of information, the publisher refused the court's request; the issue died when inspection of the negatives in private led the lawyers to withdraw their petition.[22]

IV. The Use of Cinematic Film as Evidence before the Hakata Case

Television or movie newsfilm has been used as evidence in the absence of police film or when the prosecution has considered its footage on a large scale disturbance insufficient. In acquiring film evidence, officials generally rely on voluntary presentation eased by discussions with media executives; on at least one occasion (the Diet disturbance case), film was purchased. As a rule, seizure warrants are only a *pro forma* device facilitating compliance by company executives who are sensitive about their constitutional prerogatives and their relations with media labor unions. But advances in television technology and expansion of news coverage have changed the relations between officials and the mass media somewhat, because videotape recorders are readily available, and officials can and do monitor TV newscasts.[23]

The evidentiary use of cinematic film first became an issue in the "Second Diet Disturbance" case in August, 1956. Assault charges were filed against JSP (Japan Socialist Party) legislators following a Diet scuffle, and the Tokyo district prosecutor asked three companies (*Nihon Eiga Shinsha, Yomiuri Eigasha*, and *NHK*) to submit newsfilm of the incident. Allegedly in part because of union relations, the request was initially refused; but upon issuance of a *fieri facias* order, two companies complied and the third (Yomiuri) sold film to the prosecutor's office.[24]

In relation to cases arising during the Security Treaty struggles of May and June, 1960, Tokyo prosecutors asked NHK and four commercial stations (NTV, NET, TBS, and Fuji) for evidentiary newsfilm.[25] After considerable discussion, NHK refused to cooperate, while the other companies allowed officials to view the already broadcast portions of the TV film in their respective studios.[26]

On the day of the April, 1961 Nagaoka Telephone and Telegraph Office Incident, a BSN[27] telecast carried the film of an alleged obstruction of official duties. The film was shown again during the weekend "TV Weekly News," monitored by an 8 mm. camera in a Niigata Telecommunications Bureau office, and presented on request to the district prosecutor for use as evidence. The TV record of the event was thus provided without seizure.[28]

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[19] Ishikawa, *op. cit.*, pp. 44, 45; Supreme Court, First Petty Bench, deliberations in progress at time of writing. The related high court decision is in *Daiichi Shōhōtei* No. 43 (A) 1614.

[20] Ishikawa, *op. cit.*, pp. 44, 45; Hida Teachers Union Incident, Oita Prefecture, Kyūshū, August, 1959, Fukuoka high court, May 4, 1964: 17 *Kōtōsai Hanreishū* (No. 4) 329 (1962), conviction.

[21] Ishikawa, *op. cit.*, p. 45; Yamaguchi district court, Sept. 28, 1966, conviction.

[22] Haneda International Airport, Tokyo, Oct. 8, 1967. *AS* (eve. ed.), Oct. 9, 1969; *The Asahi Evening News*, Sept. 17, 1969 and Oct. 1, 1970; *Japan Times*, Feb. 5, 1971. In early 1971, 26 of the roughly 2,000 students involved were convicted. The defense argued in vain that the students' violence was justified because they had tried to prevent Prime Minister Sato from drawing Japan into the Vietnam War.

[23] Ishikawa, *op. cit.*, p. 46, note 2, and the 1969 Kyoto University disturbances discussed in this section.

[24] Ishikawa, *op. cit.*, p. 45, 46; Masami Itō et al, "Keiji Shihō to Hōdō no Jiyū" (Criminal Justice and Freedom of Information), *Jurisuto*, No. 439, p. 18; *AS*, throughout August, 1956, and Sept. 9, 1969. It will be noted that NHK, though a publicly owned corporation, is quite independent in such cases. On Dec. 17, 1969, the Tokyo high court upheld the 1961 acquittal of the defendants while taking a dim view of Diet roughhousing. Concerning this case and the earlier 1955 Diet disturbance case, which was soon dropped for lack of evidence, see *AS* (eve. ed.), Dec. 17, 1969.

[25] Concerning the Security Treaty Crisis, see Packard, *op. cit.* NTV refers to Nippon Terebi Hōsō-mō, or Nippon Television Network Corp.; NET is Nippon Kyoiku Terebi, or Nippon Educational Television Co.; TBS, Tōkyō Hōsō, or Tokyo Broadcasting System; and Fuji, Fuji Terebi, or Fuji Telecasting Co.

[26] Ishikawa, *op. cit.*, pp. 45, 46.

[27] BSN refers to Niigata Hōsō, the Broadcasting System of Niigata.

In November, 1967, a NET telecast of the "Second Haneda Incident" on the occasion of Prime Minister Sato's return from a trip abroad was monitored by a police videotape recorder. Both defense counsel and prosecutors favored its presentation as evidence to the Tokyo district court, and clear and useful stills were subsequently extracted.[29]

During the investigation of the Yasuda Auditorium Incident (*Tōdai Yasuda Kōdō Jiken,* Tokyo University, January, 1969), 20 videotapes were presented to the Tokyo district court, without objection from defense counsel, by the NHK, NET, TBS, Fuji and Channel 12 (Japan Science Foundation) firms. Some five hundred students had been arrested on suspicion of various crimes of violence during this climactic incident in the 1968–1969 university crisis.[30]

On March 18, 1969, the Supreme Court handed down a ruling that invalidated a seizure of film.[31] Film taken by the Kokugakuin University Film Study Society during the Shinjuku (Tokyo) violence of October 21, 1968, was seized in November by Tokyo police for possible use as evidence against "AH," a member of the club. With the backing of more than five hundred politicians and "men of culture" (*bunkajin,* i.e., professors, writers, critics, and other social elites), the film society successfully sought relief in a Tokyo district court. The court held that the film was of little evidentiary value in the AH case since AH did not appear therein; that police retention of the film for use as evidence in related cases was unjustified since the seizure warrant specified only the AH case, and that the seizure had imposed an unnecessary burden on a third party (the film club).

From the night of the court's ruling on November 22, 1968, until the prosecutor's office lodged an appeal with the Supreme Court five days later, lawyers for the students vigorously demanded return of the confiscated items. The prosecutor's appeal, perhaps reflecting attitudes arising under the pre-1945 Code of Criminal Procedure,[32] claimed that investigating officers have the right to determine the necessity of such a seizure and retention of articles for evidentiary purposes without judicial obstruction in issuing or subsequently reviewing a warrant.

The Supreme Court held that seizure of third-party evidence is permissible when necessary for a criminal investigation, but also that the need for seizure must be balanced against disadvantages to the third party. The seizure in question, the court said, could not be upheld. Discussions of this case, before and after the court's decision, sometimes suggested that the central issues were the film club's freedom of expression and the propriety of seizing third-party film evidence, and thus provided encouragement to the television media. But the right of the courts rather than the prosecutors to determine the "necessity" of a seizure was probably uppermost in the judges' minds.[33]

Another case involving film use which was not perceived as inimical to media interests arose on June 21, 1969. During the videotaping of a round-table discussion, the president of Toyama University struck another participant and was charged with assault. KNB television (*Kita Nihon Hōsō,* [Kita Nihon Broadcasting Co.]) complied with the investigators' request for the film on grounds that the fracas had occurred in the company's studios. The television company also felt the film might settle the question of whether or not the aggrieved party had uttered provocative words, as alleged. During the Hakata controversy, the media referred to this case on a number of occasions as illustrating the case-by-case flexibility of their policy regarding official requests for film evidence.[34]

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[28] Ishikawa, *op. cit.,* p. 45; Niigata district court, Aug. 7, 1966; at time of writing, the conviction was on appeal in the Tokyo high court.

[29] Ishikawa, *op. cit.,* p. 45; concerning the acquittal of defendants, see *AS,* Dec. 19, 1969.

[30] Ishikawa, *op. cit.,* p. 45; *AS,* Jan. 18–23, 1969. Later in the year, unprecedented and bizarre disruptions of judicial proceedings occurred when students and defense counsel refused to appear in court except as a single body of 500, and trials of small groups were carried to conclusion in their absence. See *Hanrei Jihō* (No. 577) 47 (Jan. 21) 1970; and *AS,* almost daily, October through December, 1969. 1969 was perhaps the most eventful year in Japanese judicial history. See Beer, *"Japan, 1969 . . ." op. cit.,* p. 49.

[31] Article 43 of the Code of Criminal Procedure designates three types of judicial decision:

> "Except as otherwise provided in this law, a judgment (*han-ketsu*) shall be rendered on the basis of oral proceedings.
> 2. A ruling (*kettei*) or an order (*meirei*) shall not necessarily be based upon oral proceedings.
> 3. In making a ruling or an order, the court may, whenever necessary, make examination of facts . . ." (*Criminal Statutes,* p. 69).

March 18, 1969 ruling, Third Petty Bench, Supreme Court, *Hanrei Jihō* (No. 548) 22 (April, 1969). For a similar ruling, see *AS,* Dec. 26, 1969. Concerning seizure warrants, see Articles 218 and 430, Code of Criminal Procedure (*Criminal Statutes,* pp. 111, 160); note 44 below; and Koya Matsuo, "Kyōsei Sōsa to Shihōteki Kōsei" (Forcible Search and Judicial Restraints), *Hōgaku Seminah* (No. 159) 13 (June, 1969).

[32] Matsuo, *op. cit.,* p. 14.

[33] Matsuo, p. 14; discussions with Prof. Koya Matsuo, autumn, 1969.

[34] For example, *AS,* Sept. 9, 1969; trial in progress at time of writing.

Also in June, 1969, some students held plainclothesmen in confinement at Tohoku University (Sendai). TBC (Tohoku Broadcasting Co. [*Tōhoku Hōsō*]) presented a videotape of the event to the district prosecutor upon request. TBS, a related Tokyo company, lamented the Sendai firm's action and sought without success withdrawal of the film from evidentiary use. Defense counsel protested in vain that the court's use of the film would violate freedom of expression.[35]

Besides the Sendai incident and the *Shūkan Asahi* still photo case,[36] other exacerbating incidents took place during the height of the Hakata station film controversy. On September 24, 1969, the Kyoto district prosecutor's office submitted videotape to the district court for use as evidence against 39 students arrested in connection with the Kyoto University disturbances of September 20–21. The prosecutor had monitored news broadcasts with videotape recorders. Defense counsel denied the validity of a trial using such evidence, while local TV stations (*NHK, Asahi, Kansai TV, Mainichi,* and *Yomiuri*) issued strong protests.

A company representative claimed such practices endanger the freedom of information and could lead to refusal by "the people" to cooperate with reporters. The prosecutor's office said they would continue to monitor TV newcasts as they saw fit without the prior consent of television companies, while a Kyoto district court judge said each judge must decide whether and how the film should be used as evidence.[37]

On October 22, during the trial of students involved in the Shinjuku Incident of October, 1968, a Tokyo district prosecutor presented the court with videotapes of NHK and TBS newsbroadcasts made by investigators. Company executives visited both police and prosecutor offices, vigorously claimed violation of freedom of information, and demanded that the film be withdrawn; but it was not.[38]

V. The Hakata Station Film Case

The cases discussed above were fuel for the fire that broke out after the August, 1969 order of the Fukuoka district court (3rd Criminal Section). The court order required the four Fukuoka TV companies to present *all* film in their possession taken during the Hakata Station Incident; the mass media refused to present any of their film.[39]

The incident itself had taken place some *nineteen months* earlier, on January 16, 1968. Early in the morning of that day, about 300 Anti-Yoyogi faction[40] students from the Tokyo and Osaka areas had arrived at Hakata Station (Fukuoka, Kyushu) on their way to Sasebo for demonstrations protesting the visit of the nuclear-powered aircraft carrier, the U.S.S. *Enterprise*. When they detrained for a side visit to Kyushu University, their way was impeded for a time by some 870 riot police and railroad security personnel. Almost no injuries of any kind occurred. Four students were arrested, but the only one indicted (for interference with police duties execution) was acquitted in a Fukuoka district court on April 11, 1969.[41]

Twenty lawyers, thirty-six JSP members of the Diet, and other supporters of the students brought countercharges against the Prefectural Police Commissioner and the 870 officers for abuse of police authority. Since the district prosecutor did not allow their charges (March 25, 1969), they had recourse in the Fukuoka district court (April 4) to "an appeal from a doubtful judgment" (*fushinpan seikyū*), asking that the court try officers under "quasi-indictment" (*junkiso*) procedures.[42]

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[35] Ishikawa, *op. cit.*, p. 45. The Sendai district court ruled in favor of using the videotape as evidence on Jan. 28, 1970, *AS*, Jan. 29, 1970.

[36] See page 1125 above.

[37] *AS*, Sept. 26, 1969; *Asahi Evening News*, Sept. 25, 1969.

[38] *AS*, Oct. 25, 1969; *Tokyo Shinbun*, Oct. 23, 1969. For discussions on one group of students, see *AS* (eve. ed.), May 21, 1970.

[39] *AS*, Aug. 30, Sept. 4, 1969. For additional sources on the Hakata controversy, see notes 8, 14, and 46. A movie producer presented 1.5 minutes of 16 mm. film on behalf of the Hakata students, *Mainichi Shinbun* (eve. ed.), Sept. 25, 1969.

[40] Concerning the many factions of *Zengakuren* (National Federation of Student Self-Government Associations), see Shakaimondai Kenkyukai, ed., *Zengakuren Kakuha* (The Factions of Zengakuren), Sobasha, 1969. "Anti-Yoyogi" refers to a number of anti-Communist factions, generally ultra-leftist. For discussions of their recent activities, see Beer, "Japan 1969 . . . ," p. 43, and "Japan Turning the Corner," pp. 80–81.

[41] *Hanrei Jihō* (No. 562) 23 (Aug. 21, 1969); Koya Matsuo, "Keiji Shihō to Masu Mejia" (Criminal Justice and the Mass Media), *Jurisuto,* No. 439, p. 36. The Fukuoka prosecutor unsuccessfully appealed the April acquittal of the student, *AS* (eve. ed.), Oct. 30, 1970. On Sept. 26, 1969, lawyers for the student asked the high court to request as evidence film in the possession of a "Mr. T."

[42] *AS*, Sept. 9, 1969; *Jurisuto,* No. 439, pp. 15–18, 35–37, 51. "Quasi indictment" is the formality under which action is instituted for an alleged abuse of police authority. Since, as in the case at hand, a prosecutor may not lend support to charges (*fukiso shobun*) against police officials, postwar Japanese law provides a check and a remedy in the form of an "appeal from a doubtful judgment" (*fushinpan seikyū*) of a prosecutor to a district court, Article 262, *Criminal Statutes,* p. 122. Articles 265–269 (pp. 123, 124) provide that a favorable ruling (*kettei*) by a collegiate court on such an appeal constitutes institution of prosecution; a lawyer is chosen by the court to function

The only third-party witness to come forth was Professor Masaji Inoue (Faculty of Law, Kyushu University), who had registered a complaint on the students' behalf, with little effect, at the local Civil Liberties Bureau.[43] On May 19, while seeking grounds for a ruling on whether or not to bring the police to trial, the Fukuoka district court, at the request of the plaintiffs, asked the four Fukuoka television companies (NHK, KBC, RKB, and TNC) if they had any film of the Hakata Station Incident. After the TV companies had replied in the affirmative, the court asked for the film in a June 6 letter. The companies demurred with the comment, "The use of this film as court evidence might render free and impartial news-gathering and reporting impossible." On August 28, the die was cast when the court issued an order to submit (*teishutsu meirei*) all the Hakata film for use as evidence, and the companies countered with separate appeals to the Fukuoka High Court (September 3) and the Supreme Court (September 2).[44] Concurrently, the fourteen media companies of Fukuoka met on September 1 and endorsed the TV companies' position, as did a September 6 statement of the Shinbun Kyokai.

On September 19, the Supreme Court (First Petty Bench) quashed the special appeal (*tokubetsu kōkoku*) on technical grounds. The ordinary appeal (*ippan kōkoku*) was struck down by the Fukuoka High Court on September 20, only to be followed by a second appeal to the Supreme Court the next day.[45] All through the fall, newspapers, academic and popular magazines, radio and television stations joined in heated nationwide debate on freedom of information and judicial prerogatives.[46]

On November 26, 1969, the Supreme Court quashed the appeal in a unanimous Grand Bench decision.[47] The four TV companies, strongly backed by the Shinbun Kyokai and the NAB, had appealed principally on the following grounds: The communications media must have freedom in gathering news (*shuzai no jiyu*) because freedom of information is at the foundation of a democratic society. The Fukuoka High Court, the media said, mistakenly interpreted the constitutional provision which guarantees freedom of expression (Article 21) in judging the court order constitutional under the "public welfare" clause.[48] Even if the court had demanded only the already televised portions of the Hakata film, compliance with the order would result in the use of film for evidence—a purpose other than that for which it was intended. Such use would diminish the people's trust in the media and would lead to a loss of their cooperation and thus of the media's freedom in gathering news. Furthermore, the appellants contended, how such film is to be used must be decided by the news media themselves, and not by any outside agency. If outside compulsion were allowed even once, they charged, full freedom of information would be impossible, and the people's

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as a prosecutor in the subsequent proceedings. A negative ruling by a court may be appealed (*kōkoku*). On the average, over 100 *fushinpan seikyū* petitions have been lodged each year. A favorable ruling had been made in a total of only seven cases as of August, 1970, *AS* (eve. ed.), August 26, 1970.

In the Hakata case police were accused of "abuse of authority by public officers" (Article 193, The Penal Code, *Criminal Statutes*, p. 42); "abuse of authority by special public officers" (Article 194); "violence and cruelty by special public officials" (Article 195); violation of freedom of expression (Article 21, *Constitution*); and abuse of authority under the Subversive Activities Prevention Law (Hakaikatsudō Bōshihō, Article 45), *Roppō Zensho*, p. 911.

[43] Itō, *Jurisuto*, No. 439, pp. 15–16; Tsuneo Horiuchi, "The Civil Liberties Bureau of the Ministry of Justice and the System of Civil Liberties Commissioners (Japan)," *Effective Realization of Civil and Political Rights at the National Level: Selected Studies* (New York: United Nations, 1968), pp. 51–92; Walter Gellhorn, "Settling Disagreements with Officials in Japan," 70 *Harvard Law. Rev.* (No. 4) 685 (Feb., 1966); B. J. George, "The 'Right of Silence' in Japanese Law," in Henderson, *op. cit.*, p. 259.

[44] KBC refers to the Kyushu Asahi Broadcasting Co; RKB, the RKB Mainichi Broadcasting Corp.; and TNC, the Television Nishi-Nippon Corp. Concerning *teishutsu meirei*, see Articles 99 and 100, Code of Criminal Procedure, *Criminal Statutes*, p. 84, and note 31, above. No special punitive provisions come into play when such a court order (equivalent to a *subpoena*) is not complied with, but the court may issue a seizure warrant. Japanese courts do not have a strong contempt power like that of U.S. courts, Sakae Wagatsuma, ed., *Shinhōritsugaku Jiten*, Yuhikaku, 1962, p. 687.

[45] For the Fukuoka high court decision, see *Hanrei Jihō* (No. 569) 23 (Nov. 1, 1969); and *AS*, Aug. 30, Sept. 3, 4, 19 (eve. ed.), 20, 26, Oct. 24, 25.

[46] See notes 8 and 14 above, and the Shinbun Kyokai's weekly, *Shinbun Kyōkaihō*, beginning Sept. 2, 1969.

[47] *AS*, Nov. 26 (eve. ed.) and Nov. 27, 1969; *Hanrei Jihō* (No. 574) 11 (Dec. 21, 1969).

[48] The Constitution of Japan provides:

> "Article 12. The freedoms and rights guaranteed to the people by this Constitution shall be maintained by the constant endeavor of the people, who shall refrain from any abuse of these freedoms and rights and shall always be responsible for utilizing them for the public welfare.
>
> "Article 13. All of the people shall be respected as individuals. Their right to life, liberty, and the pursuit of happiness shall, to the extent that it does not interfere with the public welfare, be the supreme consideration in legislation and in other governmental affairs."

For the text of Article 21, see note 2 above. Concerning judicial use of the public welfare principle, see Beer, in Henderson, *op. cit.*, p. 207.

right to know (*kokumin no shiru kenri*) through full and impartial news coverage would be violated. Finally, the Fukuoka district court issued the order without really endeavoring to gather other evidence, such as eyewitness testimony.

In responding, the Supreme Court agreed that the information media in a democratic society serve the public's right to know by providing material on which to base political judgments. Consequently, the freedom to report facts, like the freedom to express ideas, is guaranteed under Article 21. Moreover, in the interests of factual reporting, protection under Article 21 must be seen as extending to the freedom to gather news.

The object of the court order at issue, the court continued, is film taken for use in news programs already presented to the public. With these telecasts, the purpose of the media's film gathering was accomplished; so the court order is not directly related to the freedom of newsgathering. By no means does this imply that use of such film for another purpose could not lead to future interference with media freedom in newsgathering.

The court held, however, that this freedom has intrinsic limits, as when it must be balanced against the constitutional right to a fair trial. The fair conduct of criminal trials is a fundamental demand made upon the State; discovery of the truth in a criminal trial is imperative. When news material of the information media is deemed necessary evidence to assure fair trial, the court said, some resultant limitation on free newsgathering may be unavoidable.

Thus, it was contended, a careful balancing of factors is required in determining whether an item is necessary evidence. Its value as evidence and the seriousness, circumstances, and nature of the alleged crime should be considered together with the degree of hindrance to free newsgathering that may result, and the extent to which this limitation will affect the freedom of information. Even when the evidentiary use of film is unavoidable, the court said, care must be taken that the disadvantages accruing to the information media do not exceed the bounds of necessity.

The deliberations of the Fukuoka district court are now at a very difficult stage for both the suspects and the accusers. As nearly two years have passed since the case arose, no new third-party testimony can be expected. The media film at issue, taken at the scene from an impartial standpoint, has very great value as evidence, and is well nigh indispensable for determination of guilt or innocence.

Since the media's purpose in gathering film was accomplished by the presentation of newscasts, it is not the freedom of reporting that is at issue but rather a fear of future obstacles to free newsgathering. Although the news media should be accorded respect, the Supreme Court held, they must endure this degree of disadvantage in the interests of fair trial. The court order was declared to be genuinely unavoidable, violating neither the letter nor the spirit of Article 21 of the Constitution, and the appeal was quashed.[49]

VI. The Aftermath of the Hakata Film Decision

After the Supreme Court decision, the four television firms remained adamant. On December 11, 1969, the three judges of the Fukuoka district court met with representatives of the four companies at the offices of TNC Television. In a ninety-minute exchange, the Chief Judge asked for compliance with the court order, but the media spokesmen replied, "We cannot voluntarily present the film. Out of consideration for the position of the information media, we want you to withdraw the order to submit the film."[50] Another meeting on February 26, 1970, produced similar results, and on March 4, a seizure warrant was issued by the court. As a conciliatory measure, the court took only the broadcast portions, rather than all the film demanded in the earlier court order. The television companies, for their part, did not resist or try to conceal the film. The media's protests aroused the sympathy but not the full support of some of their staunchest academic allies, in good part because the Supreme Court had spoken.[51] The mass media had made their point, and the courts had upheld judicial prerogatives.

On August 26, 1970, the Fukuoka district court dismissed the Hakata students' appeal on grounds of insufficient evidence, but upheld their contention that an abuse of police authority had occurred.[52] The film evidence substantiated both conclusions. Meticulous examination of the videotapes revealed that at least 38 students had been subjected to "excessive police measures" (*kajō keibi*); but the identity of the individual policemen involved was obscured by police paraphernalia and the crowd situation. The students unsuccessfully appealed their case to the Fukuoka High Court; the mass media took some comfort in the fact that no clear identifications could be made; and the police continued to defend their behavior.

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[49] On technical grounds, the tribunal also quashed appellants' contentions that the court order was both procedurally invalid and against Article 32 of the Constitution ("Article 32, No person shall be denied the right of access to the courts.").

[50] *AS* (eve. ed.), Dec. 11, 1969.

[51] Conversation with Prof. Masami Ito, March 4, 1970; *AS*, Feb. 27, March 4 (eve. ed.), March 5, 1970; and note 7, above. The court announced the following December that it would burn the confiscated film prints, *AS* (eve. ed.), Dec. 10, 1970.

[52] *AS* (eve. ed.), August 26, Sept. 2, 9, 1970. The students appealed their case on Sept. 1, 1970, citing the uncooperative attitude of the police as the major reason why identification of police was difficult. The Fukuoka High Court dismissed the appeal for want of sufficient evidence, and the case was not appealed

One of the accused, former Prefectural Police Commissioner Maeda (now President of the Police College), pointed out that policemen had not wielded night sticks or otherwise injured the students, that the politicians and lawyers supporting the students were not at the scene of the incident, and that innocent policemen and their families had lived under a pall of suspicion for over two years. The police refusal to comply with a court request for a list of all police present during the Hakata Station incident was defended by Maeda on grounds that it might violate the constitutional rights of the police. In addition, Maeda had unsuccessfully challenged (*kihi*) in court a particular judge's competence to deliberate in the district court's Third Criminal Section (the Hakata Court).[53] In short, the police were not notably cooperative with the court. Thus, an incident in which close to 1,300 people participated with no noteworthy violence resulted in a remarkable arousal of national attention and a significant quadrangular conflict involving the mass media, students and politicians, the police, and the courts.

Before examining some implications of the Hakata film controversy, we should look at two other recent film evidence cases which raise different but related issues. Workers and students who had protested the visit of the *Enterprise* to Sasebo petitioned the Nagasaki prosecutor for quasi indictment of police, as in the Hakata case. In December, 1969, the prosecutor asked five local television stations and the Kyodo News Agency for film taken during the demonstrations and was refused.[54] Lacking evidence, the prosecutor did not allow the charges against the police, and in January, 1970, an appeal (*fushinpan seikyū*) was filed in district court. In an unprecedented action, the lawyers then announced they would present as evidence videotape of an NHK newsbroadcast acquired from an unspecified commercial source,[55] to the great displeasure of NHK, since Japanese law did not provide for a property right over telecasts.[56]

On December 24, 1969, the Supreme Court unanimously quashed an appeal based on an alleged "right to one's likeness" (*shōzōken*).[57] During a Kyoto demonstration of June 21, 1962, the appellant had struck a police photographer and subsequently attempted to justify the action on grounds that the officer's picture-taking violated his constitutional right to his likeness, and thus his right to privacy. The Supreme Court (Grand Bench) recognized for the first time a constitutional right not to have one's picture taken involuntarily by police investigators unless it is necessary for criminal investigation. Under Article 13 of the Constitution, both this type of right to privacy and its limits under the public welfare clause should be recognized.[58] In the case at hand, the court concluded, the appellant illegally struck an officer gathering evidence concerning an illegal demonstration.

One implication of the court's decision is that while police may film illegal demonstrations, they are not free (as before) to photograph legal peaceful demonstrations in order to gather information concerning political activists. This, in turn, sets at least some limit on the monitoring of telecasts by officials while at the same time reaffirming their right of access to evidentiary film in criminal investigation.

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to the Supreme Court, *AS* (eve. ed.), Nov. 25 and Nov. 30, 1970; *Jurisuto*, No. 439, pp. 16 and 17; Fukuoka, *Hōritsu Jihō*, p. 90. Some constitutional lawyers suggest that State compensation should be awarded aggrieved parties in cases where an abuse of police authority is established but the identity of the guilty policemen is obscure, conversation with Prof. Naoki Kobayashi, Tokyo U., Sept. 20, 1970.

[53] The Maeda challenge was dismissed by the Supreme Court, *AS*, Nov. 20 (eve. ed.), 1969, and March 5, April 30 (eve. ed.), 1970; and *Hanrei Jihō* (No. 567) 20 (Oct. 11, 1969).

[54] *AS*, Dec. 9, 10, 16, 1969.

[55] *Mainichi Shinbun*, Jan. 13, 1970; *AS*, Jan. 11, 14, 1970.

[56] At that time no clear legal remedy existed for a TV concern if a program was videotaped and sold by another private party. The most substantial revision of copyright law in seventy years was passed by the Diet in early 1970. See *AS*, Feb. 25, 28 and throughout March, 1970; and Keiichi Yamamoto, *Chosakkenhō* (Copyright Law), Yuhikaku, 1970.

[57] The term "*shōzōken*" does not appear in any constitutional or legal provision, and was employed not by the court but by appellants. See *AS*, Dec. 24, 1969; *Hanrei Jihō* (No. 577) 18 (Jan. 21, 1970); and Michitaka Kaino, "Shōzōken to Keisatsuken," *Hōgaku Seminah* (No. 169) 2 (March, 1970). The demonstration was in violation of a local public safety ordinance (*kōan jōrei*). For Supreme Court doctrine on public safety ordinances, see Beer, in Henderson, *op. cit.* p. 220; and *AS*, July 19, 1970.

[58] For the text of Article 13, which is used as a basis both to establish and to limit rights, see note 48 above.

VII. Issues and Implications

The way in which the issues of film evidence and freedom of information have been dealt with in Japan illustrates distinctive aspects of Japan's sociopolitical system, while raising questions of wider relevance concerning constitutional and procedural law. Japan's mass media recognize the high degree of freedom they enjoy, but seem to arrogate to themselves a collective constitutional right that subordinates both a defendant's constitutional right to evidence on his behalf, and the court's authority to determine the conditions under which film may be used for evidentiary purposes.

The media maintain that the considerable resources of the police and prosecutors should provide sufficient evidence without recourse to the film of other agencies. On the other hand, mass communications firms have provided film evidence with little or no resistance in a number of instances. In some of the cases we have discussed, officials acquired media videotape or still photographs while gathering additional evidence; but in other instances media film was sought in the apparent absence of other or better evidence.

Although there was much ado about the Hakata Court order, there was no ado about the court's failure to require testimony of cameramen and other eyewitnesses from the media companies. The court's right to call such witnesses was apparently not the ground on which the courts and the media had chosen to fight; scholars, students, and police seemed to honor their wishes, although the students' cause might have been helped by more abundant eyewitness testimony.

It is noteworthy that only in the Ishii case was the "newsman's privilege" involved in a clearly justiciable way. Although not formally established in law, a "newsman's privilege" seems in large measure to be honored in practice by the courts. Without acknowledging a "media privilege," the post-*Hakata* courts may, in a somewhat analogous manner, refrain from issuing court orders to acquire film evidence. Neither courts nor media supported a distinction between broadcast (or published) and unbroadcast (or unpublished) film; but the courts may confine their requests to material already made available to the public, in view of the final outcome of *Hakata*, new videotape technology, and official practice before *Hakata*. The Hakata film case was widely recognized as a major sociopolitical controversy, but party politicians other than the JSP Socialists involved in the students' cause did not actively participate in the debate. On the other hand, media intransigence in similar cases might invite tougher legislative remedies, thus strengthening the hand of the mass media's critics.

The companies do not claim that TV newsfilm is a kind of "privileged communication" between media firms and quasi clients, to be used as evidence only with the consent of those appearing in the film. And in the film evidence cases, the question of honoring the secrecy of communications was not raised.[59] The mass media appeal principally to the general teleology of newsreporting and to assumed implications of a special relationship with "the people" or "the nation" (*kokumin*). The industry apparently sees itself as a subgroup organically related to the larger group, "the people" (not contractually related as companies to individual customers or consumers), with special privileges based on this quasi-public relationship.[60] Granted the ambiguity of the press's position in any constitutional democracy, the mass media's assumption that they enjoy the implicit trust of Japan's rather sophisticated citizenry seems a bit exaggerated, as does their fear of future noncooperation if newsfilm is used as evidence against their preferences. The media realize that news source cooperation is sometimes unpredictable, and in Japan as elsewhere, the press is occasionally guilty of sensationalism, hounding of news sources, onesidedness, and invasions of privacy which have not heightened public confidence.[61] In public and private debate on the film evidence issue, media supporters rarely seem to consider seriously the negative effects on their image of noncompliance with the court order, or the distinct possibility that the public puts more trust in the courts' judicial capacities.

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[59] Article 21, paragraph 2 of the Constitution; see note 2 above.

[60] This mode of expression, common in Japanese political debate, seems to reflect a nonmajoritarian view of democracy as based on an organic relationship between competing tutorial elites on the one hand and the general public on the other. In the case at hand, a close relationship characterized by implicit trust was assumed to exist between the people and the mass media.

[61] See, for example, the Japanese bar's criticism of press handling of the Kusano Case, *Japan Times,* March 23, 1971. In a 1964 Tokyo District court decision, the right of privacy was explicitly recognized for the first time in Japanese law, Kiyoshi Igarashi and Hiroshi Tamiya, *Meiyo To Puraibashii* (Reputation and Privacy), Yuhikaku, 1968; Hideo Niwayama, "Bekken Taiho to Higisha no Jinken" (Mistaken Arrest and the Human Rights of a Suspect), *Hōgaku Seminah* (No. 167) 11 (Feb., 1970); Fumio Ono et al., *Meiyo—Puraibashii no Saibankijun* (Judicial Standards concerning Good Name and Privacy), Sakai Shoten, 1963; and by Masami Ito (C. J. Bradshaw trans.), "Issues in the 'After the Banquet' Decision," *I. Law in Japan: An Annual* 141 (1967), *Puraibashii No Kenri* (The Right of Privacy), Iwanami Shoten, 1968, and with Michitaka Kaino, *Puraibashii No Kenkyū* (Studies on [the Right to] Privacy), Iwanami Shoten, 1962.

When media spokesmen stress the information industry's intimate relationship to the people's right to know, no mention is made of other subgroups in society, such as educators, who might refuse testimony or other evidence on grounds that it might interfere with the general trust relationship necessary for carrying on professional work. Neither do the media refer to the private profit aspects of commercial publishing and broadcasting, or to the oligopolistic structure of the information industry. Parallels have been drawn by media supporters between the desired privilege on the one hand, and State and professional secrecy privileges on the other. For example, just as the right of the State to determine what facts should be classified as state secrets is recognized in law as a public benefit often superior to fair trial, so also, it is argued, should the media's right to determine whether film shall be used as evidence be guaranteed.[62] The argument might be turned around to ask for clearer standards from both the State and the mass media.

The granting of a media privilege, either in legal formulations or by judicial practice, might well create in the private sector a quasi-judicial right that would not be subject to the strict and more easily definable limitations placed on the privileged communications of such professionals as doctors and lawyers by the Code of Criminal Procedure.[63] From the standpoint of the defendant or plaintiff (as in the Hakata case) in a criminal matter, or the party to a civil dispute desiring evidence on his behalf, the media might at times be in the anomalous position of exercising censorship over public use of published materials in a manner injurious to the individual's constitutional rights and private interests. In short, a new sort of check on judicial power would be established implying possible infringement of rights.

The mass media's position on freedom of information was influenced by such factors as a desire for independence of the industry in reporting public disturbances, fear of intrusions on editorial freedom, a strong in-group spirit, and the style of conflict that developed with the courts. Also, the issue of use of film evidence was relatively new, and the duration of postwar freedom short compared with a prior modern history of recurrent governmental press restrictions.[64]

The now powerful media companies are headed by men with a long tradition of selective opposition to the government and lively memories of pre-1945 interference with freedom of information. They are also (like their counterparts elsewhere) men who are very sensitive to the hint of an encroachment on their prerogatives; and what is more, they have a sense of self-interest, responsibility, and participation in fostering Japanese democracy. Adopting a "case by case" (*kēsu bai kēsu*) policy on the film evidence issue, the mass media maintain that their refusal in the Hakata case was predicated on political neutrality and impartiality. "Independence" might be the more apt word: independence in gathering and editing film, and independence in choosing the times and modes of the media's involvement in public controversy. For example, the media chose participation in the controversy surrounding the *Enterprise* visit, but opted against entanglement in the court cases resulting from attendant student-police confrontations. Since the mass media contributed substantially to making the *Enterprise* visit an emotion-charged national issue and brought before the public eye alleged police excesses, there may appear to be irony in media opposition to student petitions for film evidence in both the Hakata and Sasebo cases, and in their predicament with the courts. But other considerations may modify this impression somewhat.

It is clear from these and other film evidence cases that the mass media do not simply fear ungentlemanly treatment at the hands of "the people," defined as student activists.[65] Newsfilm evidence has positively and negatively affected the interests of both police and demonstrators at different times. The press was sharply critical of the rise in student violence during the unprecedented university crisis of 1968–1969, not only because this attitude expressed the consensus among influential elites but also because incidents of newsgathering obstruction by students increased notably during that period.[66] On the other hand, the media's identification of police who have obstructed newsmen has led at times to official apology and disciplinary action.[67]

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[62] For example. M. Ito, "Arasowareru 'Hōdō no Jiyū' (The "Freedom of Information" Dispute), *AS*, Sept. 9, 1969.

[63] For legal provisions concerning privileged communications, see note 12 above. The author is sympathetically inclined toward a carefully defined newsman's privilege.

[64] Concerning the history of Japanese press problems, see sources cited in note 5 above; and Yasuhiro Okudaira, "Nihon Shuppan Keisatsu Hōsei no Rekishiteki Kenkyū Yosetsu" (An Introduction to Historical Research on Japanese Publication Regulations), *Hōritsu Jihō*, seven articles, April through October, 1967; Peter Figdor, "Newspapers and their Regulation in Early Meiji Japan, 1968–1883," unpublished paper, Harvard U., June, 1969; Kanesada Hanazono, *The Development of Japanese Journalism*, (Osaka: The Osaka Mainichi, 1924); Harry E. Wildes, *Social Currents in Japan: With Special Reference to the Press* (Chicago: University of Chicago Press, 1927).

[65] Note, for example, the differences in perspective of Tomokatsu Maeda (a leading lawyer for students) and Toshio Yamada (a Shinbun Kyokai executive) in Ito, *Jurisuto*, No. 439, 15–34.

Over the years, however, the mass media have been both more critical and more cautious of police than of students. If media attitudes toward the Hakata and Sasebo disturbances are juxtaposed with media behavior in the related film evidence cases, a similar pattern emerges. Police units suspected of abusing their authority were criticized by the news media, but the press did not willingly help the courts identify individual policemen.

In repeated requests for newsfilm evidence from various quarters (lawyers, prosecutors, judges), and especially in official monitoring of telecasts, the mass media may perceive an indirect threat to their editorial independence, in a nation notable for subtle but significant change.[68] Given the media's desire to determine which politically charged trials they become involved in, the unrestrained use of readily available newsfilm as evidence might lead TV film editors to refrain from telecasting footage that would be most useful as evidence. The frequent employment of unused portions of newsfilm as evidence might dictate self-imposed but unwanted media constraints on film gathering during public disturbances.

The capacity of the mass media to respond in unison to the perceived threat from outside the industry is itself notable.[69] A basic feature of Japan's highly organized society is the ability of otherwise competing groups to combine into a solid united front when the interests of a system of related groups are threatened by "outsiders." The underlying assumption is that the benefit of the individual (or the group) is achieved only by working for the good of the group (or the configuration of groups). With a shift in the parameters of one's ingroup from a smaller to a larger unit, loyalty becomes focused on the larger ingroup to the extent and for as long as relations with "outsiders" dictate. Factions within a political party and subdivisions within a business firm, Ministry, or news media company may compete freely among themselves, but unanimous loyal support of the larger unit is normally expected in dealings with "outsiders."

Applying this generalization to the Hakata film case, we see that each TV company was an in-group competing with other firms until the court asked for evidentiary film. At that point the four Fukuoka companies formed a coherent group vis-à-vis the district court. When the court order was issued, the entire news industry became a well nigh single-minded group, unified in opposition to the court system and to other "outsiders" who opposed their position. Other considerations were radically subordinated to the desire for the media privilege. Dissent from the media's position on the part of individuals within the industry might well be read by colleagues as mistaken and disloyal; but the freedom of the individual to dissent (or to testify, in the case of the cameramen recording the Hakata incident,) was not commonly an issue, since a consensus had been reached by the group. Similarly, the many policemen who were present at Hakata Station but not engaged in illegally obstructing the students' passage would have found it very hard to testify freely in court against fellow policemen with whom they would have to spend all their remaining working years. (Limited job mobility intensifies ingroup ties in Japan.)

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[66] For examples, see an incomplete and unpublished report on instances of obstruction to *Asahi Shinbun* cameramen, principally during 1968 and 1969. The report describes 60 cases, 39 of which involved students, and 15 the police, "Asahi Shinbun Shashinbuin (Kameraman) ga Koko Sūnenkan ni Demoshuzai de Uketa Shuzai Bōgai, Bōkō, Shōgai nado no Jitsurei" (Instances of Obstruction . . . to News-gathering during Demonstrations Experienced in Recent Years by Asahi Shinbun Cameramen), Asahi Shinbun Photography Department, Sept. 25, 1969.

[67] For a recent example, see any major Japanese newspaper, May 17–21, 1970.

[68] Note, for example, the repressive implications attached to slight shifts in postwar educational policy during the Ienaga textbook trial, and the modes of intensifying nationalism in prewar Japanese education. For Saburo Ienaga's perspectives, see his "The Historical Significance of the Japanese Textbook of Lawsuit," *The Bulletin of Concerned Asian Scholars,* 2 (Fall, 1970), pp. 2–12. For background, see John Caiger, "Education, Values and Japan's National Identity: A Study of the Aims and Content of Courses in Japanese History, 1872–1963," unpublished doctoral thesis, Australian National University, Canberra, 1966. The district court decision was in favor of Ienaga. See *Jurisuto,* special issue, No. 461 (Sept. 5, 1970) and *Hōritsu Jihō,* special issues, No. 486 (Aug., 1969) and No. 501 (Sept., 1970); and Beer, "Japan Turning the Corner," pp. 78–79.

[69] The dynamics of this type of ingroupism which is open to larger loyalties seems one key to understanding Japan's modern nationalism and successes. See Chie Nakane's *Japanese Society* (Berkeley: University of California Press, 1970); Beer, in Henderson, pp. 210–220; Kazuko Tsurumi, *Social Change and the Individual: Japan before and after Defeat in World War II* (Princeton: Princeton University Press, 1970); Chikio Hayashi *et al., Nipponjin no Kokuminsei* (A Study of Japanese National Character), Vol. 2, Institute of Statistical Mathematics, Shiseido, 1970; Japan Regional Development Center, ed., *Nihonjin no Kachikan* (The Sense of Values of the Japanese People), Shiseido, 1970; and Peter A. Corning, "The Biological Bases of Behavior and Some Implications for Political Science," *World Politics* 23 (April, 1971), esp. pp. 327–328.

In the Hakata film case, the manner in which the district court dealt with the television companies was important. An unprecedented flat order was issued directing them to present *all* the film of the Hakata Station Incident then in their possession. The unyielding position taken by the media's spokesmen was due in part to their belief that the district court had eschewed quiet, informal persuasion until after the Supreme Court decision of late November. In the media view, all the normal societal mechanisms for settling disputes had not been employed with sufficient patience and finesse. While the reader may feel that the time lapse of more than three months between the court's initial inquiry concerning the film and its issuance of the court order reflected unusual judicial moderation, the direct confrontation created by the court order was seen as bad form and somehow excessive (*muri*) by many in a nation where polite indirection and informal conciliation often serve to reduce the embarrassments of formal public conflict.[70] Where the raw edge of power struggle is not smoothed off in Japan by patient negotiations leading to some measure of consensus, a state of restrained intergroup warfare develops, testing the real, not formal, power of each protagonist.

In the face of open challenge, the mass media closed ranks, adopted a rigid position, struggled vigorously with the Fukuoka district court, and of course yielded to the court's quiet but more substantial power. But defeat itself was appropriately softened by the contents of the court's seizure warrant—the already broadcast portions of the Hakata film are specified in the warrant, but no mention is made of the unbroadcast footage hitherto demanded. Both because it is at least temporarily necessary for face-maintenance and because it provides a rationale for the independence of the media in the complexities of Japan's sociopolitics, the media privilege will probably be claimed without significant modification for the indefinite future. In the absence of legislation giving formal legitimacy to their claim, the mass media will appeal to the ultimate authority of the Constitution and "the people," and will call upon the judiciary to provide implicit, informal recognition of the media privilege by refraining from allegedly wrong-headed orders.

The issues of media freedom and criminal justice in Japan call attention to the important role of law in channeling and facilitating, rather than damming up the flow of a society's informal dispute-settlement procedures. It is still unclear what actual effects the regular use of media film for evidentiary purposes will have upon investigation, court procedures, and the gathering of news about Japan's perennial demonstrations. Might this practice lead to less reliance on eyewitness testimony, in a nation where citizens shun involvement in others' disputes as it is, where public controversies are often viewed as private inter-group conflicts,[71] and where film evidence is readily available through monitoring devices? Will officials simply expand their own film gathering capacities, or continue to rely upon media telecasts? How substantially could systematic manipulation of film shooting and editing affect freedom of information and the already limited utility of cinematic film as evidence?

Post-1945 constitutional changes and telecommunications technology have given rise in Japan to these new issues. Japan and other technologically advanced democratic systems must ask: What legislative and judicial policies should be adopted regarding the use of film as evidence, in the absence of better evidence, or in the face of popular or media unwillingness to cooperate with the courts? What sorts of regulation of police, prosecutors, media, and/or courts are compatible with criminal justice and freedom of information?

While the mix of law and sociopolitics in Japan (as in any nation) is unique in some respects, it may nevertheless provide grist for mental mills working on the adjustment of technological innovations to the exigencies of the decades ahead.

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[70] A well-informed media executive suggested to the author that the TV companies would have been less intransigent had the Fukuoka court dealt with them in a more respectful and conciliatory manner. Concerning conciliation thought and practice, see Dan Fenno Henderson, *Conciliation and Japanese Law: Tokugawa and Modern* (Seattle: University of Washington Press, 1965); and Takeyoshi Kawashima, "Dispute Resolution in Contemporary Japan," in Von Mehren, *op. cit.*, p. 41.

[71] These factors may explain in part why a jury system, though once provided for in law, was not accepted; see Von Mehren, *op. cit.*, pp. 21–22, 318–319.

The Municipal Voter: Voting and Nonvoting in City Elections

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An anomalous deficiency of the voluminous voting behavior literature is its almost exclusive preoccupation with national presidential elections although the bulk of voting in the United States, if not in Europe, occurs in local elections. Curiously, this neglect has occurred even though the granddaddy of voting research —Merriam and Gosnell's herculean investigation of the causes of nonvoting—was a study of the Chicago city election of 1923.[1] Merriam's and Gosnell's call for similar studies of elections in other cities was not widely heard. Subsequently, there have been a few investigations of the electorates of national and state elections in specific cities,[2] but only one reported study of a city election,[3] and none employing survey research. This paper, a survey analysis of municipal voting, is a response to Merriam and Gosnell's forgotten call.

A valuable investigation of the correlates of turnout in city elections has demonstrated that type of election, form of government, region, age of city, mobility, educational level, and ethnicity are significantly correlated with aggregate turnout rates.[4] The ecological method can tell us little, however, about the characteristics of individual voters, the composition of the *de facto* municipal electorate, or how it compares with that of the national electorate.

Perhaps the fundamental reason for this lacuna is an assumption that local electoral behavior is hopelessly idiosyncratic, but the Alford and Lee ecological analysis and two case studies indicate that there are some regularities in turnout patterns of city elections, although the matter is obscured by methodological differences. Demographic data from official records were used by Martin to compute voting rates of categoric groups among "qualified electors" in an Austin, Texas election in 1933. Most of those measurements do not synchronize with this study, which employs survey method and the variables and categories of the national election studies, but the findings of both are strikingly congruent. Responsibility for the similarity in the turnout patterns we would attribute to the one fundamental common property of those elections: low turnout. With this evidence that the 1967 Toledo election was fairly typical, we shall compare our findings with those of the national voting studies.

Obviously, some important dimensions of presidential voting are not duplicable in the nether regions, and local diversity circumscribes the range of generalizations. Nevertheless, a few known regularities characterize city elections. The two paramount regularities are that city elections are predominantly nonpartisan and that turnout is relatively and consistently low. This study is an exploration of some implications of those two salient characteristics of municipal elections.

Hypotheses

The following propositions regarding turnout have been demonstrated by the various presidential elections studies ranging from the Erie County study of 1940[5] to recent investigations: Electoral participation is a function of an individual's psychological involvement. Three particularly important dimensions of psychological involvement are degree of interest in politics, sense of political efficacy, and strength of party identification—these being the strongest correlates of presidential voting.[6] Some demographic

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* The writer is indebted to Mrs. Duane Stranahan and the Bowling Green University Foundation for financial assistance in securing field data.

[1] Charles E. Merriam and Harold F. Gosnell, *Non-voting, Causes and Methods of Control* (Chicago: University of Chicago Press, 1924).

[2] James K. Pollock, *Voting Behavior: A Case Study* (Ann Arbor: University of Michigan Press, 1939); Edward H. Litchfield, *Voting Behavior in a Metropolitan Area* (Ann Arbor: University of Michigan Press, 1941); Clifton McCleskey and Dan Nimmo, "Differences Between Potential, Registered, and Actual Voters," *Social Science Quarterly,* 49 (June, 1968), 103–114.

[3] Roscoe Martin, "The Municipal Electorate: A Case Study," *Southwestern Social Science Quarterly,* 14 (December, 1933), 193–237, reprinted in *Social Science Quarterly,* 50 (March, 1970), 890–936.

[4] Robert P. Alford and Eugene C. Lee, "Voting Turnout in American Cities," *American Political Science Review,* 62 (September, 1968), 796–813.

[5] Paul Lazarsfeld, Bernard Berelson, and Hazel Gaudet, *The People's Choice* (New York: Duell, Sloan, and Pearce, 1944).

[6] Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* (New York: Wiley, 1960), ch. 5; Bernard R. Berel-

variables also operate rather consistently. Men vote more frequently than women,[7] whites more frequently than blacks even outside the South,[8] and voting rate increases with age up to about 55, when it begins receding.[9] Presidential voting correlates substantially and very consistently with the principal socioeconomic variables—education, occupation, and income[10]—but education does not correlate with turnout when interest is controlled,[11] nor when economic status is controlled.[12] Reference groups also are related to participation: Unionists vote more than nonunionists, and Catholics more than Protestants.[13] The socioeconomic factors combine to produce higher voting by Republicans than Democrats.[14] Although interest is the paramount correlate, more people vote in presidential elections than are actually interested in politics.[15]

Relating these propositions to the basic characteristics of city elections—that is, low turnout rates and nonpartisanship—we can frame the following hypotheses about the *de facto* municipal electorate:

(1) Most of the psychological, demographic, and socioeconomic variables associated with turnout in presidential elections will have a more pronounced association in city elections, because of the weakness or complete absence of the counterforces which dampen them in presidential elections—the dramaturgy and charisma, appeals to civic duty, party loyalty, and the shooing of the apathetics to the polls by the party organizations.

(2) The differential between Democrat and Republican turnout will be greater in municipal than presidential elections, because status variables will have greater relative impact in the absence of most of the counterforces of presidential elections.

(3) The association of voting rate and strength of party identification usually will be lower in city elections, but there is likely to be some such association even in nonpartisan elections, because strong party identifiers probably have greater interest in public affairs than weak identifiers. Where parties actively support candidates in officially nonpartisan elections, the association of turnout and strength of party identification may be different among Democrats and Republicans, if the adherents of one party are more responsive to party slating than are the adherents of the other party. This variable probably is the least predictable and the most likely to be affected by the type and circumstances of city elections.

(4) Two correlates of presidential voting will be weaker in municipal elections. There should be scant difference between Catholic and Protestant turnout, but the Protestant rate may be slightly higher because of the strong association of turnout with social status.

(5) Likewise trade union affiliation should be a weak or negligible variable in city elections.

(6) In municipal contests, turnout rates will be somewhat higher for partisan than for nonpartisan elections.

These hypotheses certainly are not esoteric. Textbooks are sprinkled with undocumented speculation in this vein. The purpose of this paper is to check the validity of these hypotheses against data from the September, 1967 "primary" in Toledo. A citywide sample of 460 interviews conducted during the week preceding the primary and simultaneous open housing referendum, and subsequent scrutiny of election records to ascertain which respondents voted, provide a snapshot of the *de facto* electorate.[16] Comparable data gleaned from the Merriam and Gosnell study of nonvoting in Chicago reveal some significant developments in the intervening 44 years.

Psychological Involvement

The authors of *The American Voter* support the proposition that electoral participation is a

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son, Paul F. Lazarsfeld, and William N. McPhee, *Voting* (Chicago: University of Chicago Press, 1954), p. 26.

[7] Lazarsfeld *et al., op. cit.,* p. 48; Angus Campbell and Robert L. Kahn, *The People Elect a President* (Ann Arbor: Institute of Social Research, 1952), p. 38; Angus Campbell, Gerald Gurin, and Warren E. Miller, *The Voter Decides* (Evanston, Ill.: Row, Peterson, 1954), ch. 5.

[8] Campbell *et al., The Voter Decides.*

[9] Campbell *et al., The Voter Decides; Statistical Abstract of the United States, 1968,* p. 372.

[10] Lazarsfeld *et al., op. cit.,* ch. 5; Campbell *et al., The Voter Decides,* ch. 5.

[11] Sheldon J. Korchin, "Psychological Variables in the Behavior of Voters," (Unpublished doctoral dissertation, Harvard University, 1946), cited in Berelson *et al., op. cit.,* pp. 331, 333.

[12] Lazarsfeld *et al., The People's Choice,* p. 47.

[13] Campbell *et al., The Voter Decides,* ch. 5. This is an artifact of low voting in the South.

[14] Campbell *et al., The Voter Decides,* p. 109.

[15] Berelson *et al., Voting,* p. 31.

[16] Two hundred eight clusters of three housing units were drawn systematically. A randomizing device was used to spot the cluster within each block and another to select the interviewee. Interviews were completed in all but one cluster, two in most, with a completion rate of 77 per cent, exclusive of vacancies and razed areas. A current and remarkably accurate population estimate provided by the Toledo Planning Commission shows that the voting rate in the primary was 35.7 per cent of the adult population. The rate in the sample was 35.6 per cent.

function of psychological involvement by demonstrating that the three strongest correlates of voting in presidential elections are an individual's interest, his sense of political efficacy, and the strength of his identification with a political party. We hypothesize that in city elections, and particularly the nonpartisan ones, the correlation of turnout and strength of party identification will be less, but the correlation with political efficacy and interest will be greater than in presidential elections.

As Table 1 shows, the pattern of association is the same in city and presidential elections, but the starting plateau is much higher for presidential elections, which attract even a majority of persons at the bottom of the interest and efficacy scales. Is our hypothesis refuted? No, but it must be revised. Observe that the turnout ranges for degree of interest are about equal in both elections, but the Low/High ratios are 4/9 and 6/9 respectively. Conclusion: the influence of interest and political efficacy are comparably potent in both classes of elections, but their impact may be proportionately greater in low turnout city elections.

The gap between voting rates of Democrats and Republicans (see Table 2) was relatively much greater in the Toledo primary than in presidential elections—2 to 3 rather than 6 to 7. The Republican turnout rate has kept the GOP "in the ball game" nationally since 1930, and it may be even more significant in city elections.

The most striking aspect of Table 2 is the inconsistent association of turnout and party identification.[17] The voting rate of Strong Republicans in the Toledo primary was double that of Weak Republicans, but there was no difference among Democrats, who voted even less than Independents, another surprising contrast to presidential elections. There is, however, a correspondence between the two elections: in both there is more association between turnout and party identification among Republicans than Democrats. Evidently that differential was augmented by the circumstances of the Toledo case, a very low turnout nonpartisan election of Adrian's intermediate type in which party slates were important. The data suggest that the markedly superior turnout rate of Republicans was due not only to status variables and civicism, but also to greater Republican than Democratic awareness of the partisan dimension.

Table 1. Psychological Involvement and Turnout

Degree of Interest	Percentage Voting		Sense of Political Efficacy	Percentage Voting	
	Toledo Primary	1956 Pres.* Election		Toledo Primary	1956 Pres.* Election
Low	19	58	Very low	27	52
Medium	30	72	Low	37	60
High	45	87	Medium	—	75
			High	35	84
			Very high	41	91

* *The American Voter*, 103, 105.

Demographic Variables

Of the demographic variables, age most conspicuously conforms to our general hypothesis. The age curve of the Toledo primary has distinctly more arc than that of presidential elections; most young "voters" and retirees opted out. The sex differential was only slightly larger than in presidential elections, enough however, that unlike recent presidential elections, the Toledo primary attracted a slight majority of male voters. On its face, Table 3 indicates that the race differential is equal in city and presidential balloting; the figures may be misleading, however, because the presidential data are affected by the South, and in 1967 Toledo Negroes had the extraordinary incentive of an open housing plebiscite.

Table 2. Party Identification and Turnout

Party Identification	Percentage Voting	
	Primary	1952 Pres. Election*
Strong Democrat	31	65
Weak Democrat	32	59
Independent	36	64
Weak Republican	29	66
Strong Republican	58	78
Democrats	31	62
Republicans	46	72

* *The Voter Decides*, 109; adjusted to remove inflation from "overreporting."

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[17] The turnout rates for presidential elections in Tables 2, 3, 4, and 6 are adjusted to correct for the notorious "overreporting" of voting rates in post-election surveys. Survey data of different presidential elections were used to achieve the best matching of categories for each variable. The adjustments were as follows:

Voting Rates (percentage)	Presidential Elections		
	1948	1952	1964
Reported by survey	63.6	73.5	69.3
Reported by *Stat. Abstract*	51.3	62.6	63.0
Correction factor	.807	.851	.910

Table 3. Demographic Variables and Turnout

	Percentage of Adults Voting		
	1923* Chicago Election	1967 Toledo Primary	1964** Pres. Election
By sex			
Men	51	39	65
Women	28	33	61
By race			
White		37	64
Black		29	53
By age			
21–24			47
21–29	44	17	
30–39	40	33	
24–44			63
40–49	37	43	
50–59	37		
50–64		51	
45–64			69
60 up	33		
65 up		33	62

* Calculated from data in *Nonvoting*, 7, 30.

** *Statistical Abstract of the United States, 1968*, 372; adjusted to remove inflation.

Turnout in the 1923 Chicago election, computed by relating Merriam and Gosnell's sample of 6,000 nonvoters to the 1920 census, shows some interesting contrasts, some developments, and some continuities. Then the sex differential was tremendous and turnout correlated negatively with age! A third fundamental difference was the large volume of nonvoting because of alienage—360,000 in Chicago, one-fifth of the adult population and one-third of the nonvoters.

Reference Groups

We have hypothesized that reference group membership comprises a category of variables which have less association with turnout in city than in presidential elections. In fact, we assumed that such affiliation would have negligible influence in city elections, but that Protestants might vote more frequently than Catholics. We have found evidence that party identification may influence turnout even in nonpartisan systems. No qualifications of the hypothesis are needed for religion and unionism; both had scant relationship to turnout in Toledo, although Protestants did vote slightly more than Catholics. The insignificant margin of unionist voting in the Toledo primary is striking in view of the size of unions in Toledo, the political consciousness of the leaders, and their slating activity.

Status Variables

The strong association of status variables and turnout is one of the most prominent regularities of presidential elections, although the magnitude varies somewhat with aggregate turnout. The Toledo data indicate how potent they are in low-turnout city elections. An SES index was constructed by trichotomizing education, occupation, and income. Sixty-one per cent of respondents at the top of the SES scale voted, compared to 26 per cent of those at the bottom. The tremendous impact of status is demonstrated by the incongruity of the composition of the *de facto* electorate and the adult population. The mean SES index was 3.6 for the sample, 4.6 for the voters, and 3.1 for the nonvoters. The nonvoters were more representative of the populace than the voters were.

Observing that the turnout ranges for status variables were about the same amplitude in Toledo as in presidential elections conveys an impression that impact is equal at both levels. That is not correct, because the range for presidential elections is above the Toledo range. The low plateau made the impact of status variables greater in Toledo. Thus, the ratio of turnout of unskilled workers to that of professional and managerial occupations was 2/3 in the presi-

Table 4. Reference Groups: Church and Union

Religious Preference	Percentage Voting			Union Membership	Percentage Voting		
	1967 Toledo Primary	Presidential* Elections of			1967 Toledo Primary	Presidential* Elections of	
		1948	1952			1948	1952
Catholic	34	64	72	Members	36	59	65
Protestant	36	47	61	Nonmembers	35	50	62

* *The Voter Decides*, 71, 73; adjusted to remove inflation.

dential election but 2/5 in the Toledo primary. The impact of status is proportionally much greater in low-turnout elections.

The association of turnout and each of the status variables is about equal. Are they equally causal? One might anticipate that education is more so, because it is a potent molder of many attitudes and the more years of formal education the greater the exposure to the norms of civic culture. But here is a classic instance of spuriousness. Controlling the Toledo data for either occupation or income extinguishes the correlation of education and turnout.[18]

An obvious reason for hypothesizing lower turnout in nonpartisan than partisan city elections is that to the extent the nonpartisan objective obtains, two major voting stimuli are removed—subjective feelings of party loyalty and the activity of party apparatus. There also are concomitants of nonpartisan city elections which logically do not foster turnout. Nonpartisan election is bound up with other components of the municipal reform model of city government, such as at-large election of councilmen, which produces a long ballot and a heavy informational burden on the electorate. Another component is the norm of de-politicized decision making. To the extent that the goal of expelling "politics" in favor of a sanitized decision making process is realized, by-products may be issueless elections and name lotteries.[19]

The association of electoral system type with turnout is rather slight for large cities but substantial for others. Of the eighteen largest cities, the mean turnout of mayoralty elections in the 1948–1952 period was 42.0 per cent for those with partisan elections compared to 38.4 per cent for the nonpartisan ones.[20] But a 1962

Table 5. SES and Turnout Toledo Primary

SES Level	Percentage Voting	SES Level	Percentage Voting
1	26	5	52
2	34	6	41
3	33	7	61
4	36		

Mean SES

Voters 4.6 Nonvoters 3.1

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[18] The association of education and turnout in congressional and presidential elections also has been found to be epiphenomenal. Stephen E. Bennett and William R. Klecka, "Social Status and Political Participation: A Multivariate Analysis of Predictive Power," *Midwest Journal of Political Science*, 14 (August, 1970), 355–382.

[19] This theme is elaborated and documented in Howard D. Hamilton, "Costs of Reform," *National Civic Review*, 58 (November, 1969), 469–475.

[20] Calculated from a table in Edward C. Banfield and James Q. Wilson, *City Politics* (New York: Random House, 1963), p. 225, classifying Chicago as partisan.

Table 6. Status Variables

Years of Education	Percentage Voting: 1967 Toledo Primary	Percentage Voting: 1964* Pres. Election	Occupation	Percentage Voting: 1967 Toledo Primary	Percentage Voting: 1952** Pres. Election
8 or less	27	54	Unskilled	21	51
9 to 11	34	60	Skilled & semi.	31	63
12	37	69	White collar	46	64
over 12	43	77	Prof. & mgt.	50	76

Income	Percentage Voting: 1967 Toledo Primary	Percentage Voting: 1964* Pres. Election
Below 3,000	26	48
3,000– 5,000	29	57
5,000– 7,500	31	66
7,500–10,000	34	71
Over 10,000	53	77

* *Statistical Abstract of the United States, 1969*, 372.
** *The Voter Decides*, 72; adjusted to remove inflation.

survey of cities above 25,000 population disclosed a median rate of 50 per cent for the partisan election cities and 30 per cent for the nonpartisan cities, and 41 per cent and 27 per cent respectively for elections not coincident with state or national elections.[21]

Conclusion

This profile of the electorate at a low turnout nonpartisan election of a medium-size city generally conforms to hypotheses about the characteristics of the municipal electorate that are the logical corollaries of two conspicuous features of city elections, low turnout and nonpartisan form. The association of turnout rate and social status was far greater than in presidential elections, with a low-high ratio of 1 to 2 compared to 5 to 7, because status was less offset by the external counterforces that establish a higher plateau of voting in presidential elections. Thus the participation rate of people at the bottom of the status scale is approximately one-half in presidential elections but was only one-fourth in the Toledo primary. By controlling we found that vocation and income were stably related to turnout, but the association of voting and education was epiphenomenal. Consequently, the differential between voting rates of Democrats and Republicans was much larger than in presidential elections. As hypothesized, no significant association was noted between turnout and affiliation with church or labor union.

A prominent contrast between this city election and presidential ones is the relative strength of classes of variables. Psychological variables are the strongest correlates of presidential voting, but in the Toledo election social status was the strongest correlate, and interest was the only psychological variable with a higher association than party preference. The data only partially support our hypotheses about the impact of the psychological variables in city elections; there was greater correspondence to the patterns of presidential elections than we anticipated. The association of turnout with degree of interest and sense of political efficacy was about the same as in presidential elections. The association with strength of party identification, which is greater among Republicans than Democrats in presidential elections, was in this instance much sharper among Republicans and nonexistent among Democrats. Evidently, turnout level is related to this variable differentially.

The association of demographic variables also correspond to presidential election patterns, but age was far more significant. The low-high ratio was 1 to 3 compared to 5 to 7 in presidential elections. The sex difference was slightly greater and the race difference was slightly less than in the 1964 presidential election. Race played a special part in the Toledo election because of a simultaneous open housing referendum. Logic suggests, and comparison of racial voting rates in the 1964 and 1966 national elections indicates, that a consequence of low turnout in city elections is likely to be a wider gap between white and black voting rates.

Our general hypothesis requires revision, because only three variables—social status, age, and party preference—displayed a substantially greater amplitude in the Toledo primary than in recent presidential elections. Our general proposition now becomes: Most of the psychological, demographic, and socioeconomic variables display comparable amplitudes in city and presidential elections, but their impact is much greater in city elections because the low voting level makes their impact proportionately greater.[22] Therefore, the composition of the *de facto* municipal electorate is far less representative of the populace than is that of the presidential electorate.

This profile of a city electorate indicates that the low turnout for city elections has consequences that are more than academic or symbolic, and that the insulation of municipal political systems from national politics has latent functions. If Mr. Dooley was correct that "politics ain't beanbag," the divergence of the *de facto* municipal electorate from the composition of the community may affect "who gets what, when, and how" and may be related to current urban unrest.

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[21] Eugene C. Lee, "City Elections: A Statistical Profile," *1963 Municipal Yearbook* (Chicago: International City Managers' Association, 1963), pp. 74–84.

[22] The data support Tingsten's "law of dispersion," i.e., that socioeconomic differences in turnout decline as the overall level of participation increases. Herbert Tingsten, *Political Behavior* (Totowa, N.J.: Bedminster Press, 1963), p. 230.

Pareto Optimality in Electoral Competition*

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Professor Martin Shubik, in a recent essay,[1] models a competitive election as a two-person zero-sum game in which (1) the players are the two candidates, (2) a strategy is a state of the economy or a rule for selecting a state, (3) the payoffs are the candidates' pluralities, and (4) voters are utility maximizers. With this approach, he attempts to demonstrate that markets achieve Pareto optimal states, while, for equivalent conditions, an election is inefficient. We conclude, however, that, in its abstract description at least, an election is also efficient. First, we distinguish between pure, risky, and mixed strategies, demonstrating that Shubik's analysis entails a confusion among these categories. Second, we prove that if this distinction is properly applied, the outcome of a competitive election between two candidates is Pareto optimal. As a caveat to this analysis, however, we conjecture that in, say, the debate over a constitution in which future sequences of elections are being considered, citizens may unanimously prefer a market rather than an electoral mechanism.

We begin with some definitions. First, we define a state of society or a state of the economy as a description of the goods possessed by each citizen. Next, we define a *pure strategy*, θ, as a proposed state. Thus, to say that candidate 1 adopts the strategy θ is to say that he proposes to institute or to work for the state of society θ. A *risky strategy*, say $P(\theta)$, is a probability distribution over some set of pure strategies that the candidate (implicitly or explicitly) tells the electorate he will use in choosing a specific θ if elected.[2] That is, the candidate publicly adopts $P(\theta)$ as his strategy before the balloting, and then if elected, selects a particular θ probabilistically. Finally, a *mixed strategy*, say $p(\theta)$, concerns the mechanism by which some pure strategy is selected prior to the election: The candidate selects a pure strategy probabilistically before the balloting according to the rule $p(\theta)$, and then presents this pure strategy to the electorate for approval. The distinction between mixed and risky strategies, then, entails the distinction between choosing a pure strategy probabilistically before and after the balloting. Thus, if both candidates adopt mixed strategies, citizens choose between pure strategies, whereas if both candidates adopt risky strategies, citizens choose between these risks.

Turning now to the concept of Pareto optimality, a state of society or a probability distribution over states of society is *Pareto optimal* if there exists no other state or distribution such that the welfare or expected welfare of at least one citizen can be increased without diminishing the welfare or expected welfare of someone else.

To evaluate elections with the criterion of Pareto optimality we must, of course, ascertain the meaning of an electoral outcome. But we must first define the concept of an electoral equilibrium and second state our assumptions about candidates and citizens. Suppose that S_1 and S_2 are strategies for candidates 1 and 2 respectively (disregarding for the moment whether these are pure, risky, or mixed strategies) and let $\phi^1(S_1, S_2)$ and $\phi^2(S_1, S_2)$ be the candidates' expected pluralities. The strategy pair $(S_1{}^*, S_2{}^*)$ is an *equilibrium strategy pair* if

$$\text{(1a)} \qquad \phi^1(S_1{}^*, S_2{}^*) \geq \phi^1(S_1, S_2{}^*)$$

$$\text{(1b)} \qquad \phi^2(S_1{}^*, S_2{}^*) \geq \phi^2(S_1{}^*, S_2)$$

for all S_1 and S_2. That is, if 2 adopts $S_2{}^*$, candidate 1 does at least as well with $S_1{}^*$ as with any other strategy, and he may do better. Observe, moreover, that since ϕ^1 and ϕ^2 are expected *pluralities*, $\phi^1 = -\phi^2$. Thus, (1) can be restated as,

$$\text{(2)} \qquad \begin{aligned} \phi^1(S_1{}^*, S_2) &\geq \phi^1(S_1{}^*, S_2{}^*) \\ &\geq \phi^1(S_1, S_2{}^*) \end{aligned}$$

To illustrate the meaning of expression (2), suppose that $\phi^1(S_1{}^*, S_2{}^*) = 0$, which is to say that if both candidates adopt their equilibrium strategies, the expected outcome is a tie. Expression (2) states then that $S_1{}^*$ guarantees candidate 1 the expectation of a tie, that no other strategy guarantees him a higher expectation, and that he may attain a positive expectation with $S_1{}^*$ if his opponent fails to adopt $S_2{}^*$.

In choosing assumptions about candidates and

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[1] "Voting, or a Price System in a Competitive Market Structure," *American Political Science Review*, 64 (March, 1970), 179–181.

[2] For a spatial analysis of risky strategies, see Kenneth Shepsle, "Parties, Voters, and the Risk Environment," in Richard G. Niemi and Herbert F. Weisberg, eds., *Probability Models of Collective Decision-Making* (forthcoming). Pure strategies are a special case of risky ones: $P(\theta) = 1$ for one value of θ and equals zero otherwise. It is useful, however, to maintain the distinction in this essay because of the apparent confusion between risky and mixed strategies.

citizens, we can, of course, adopt the ones Shubik uses. Briefly, these assumptions are that: (i) both candidates are free to choose from identical sets of strategies, and (ii) a citizen's evaluation of a candidate is based solely on the strategy that the candidate adopts publicly, and the citizen votes for the candidate that yields him the greatest utility or expected utility.[3] To evaluate these assumptions we state their implications in terms of (2). Briefly, (i) and (ii) define a symmetric election: If the candidates adopt identical strategies, the expected outcome is a tie (i.e., $\phi^1(S, S)=0$); if they adopt different strategies and then switch, ϕ^1 and ϕ^2 simply change sign, and; if S^* is an equilibrium strategy for one candidate, it is also an equilibrium strategy for the other candidate. Some might object, then, to the use of (i) and (ii) in the belief that they render the analysis trivial and uninteresting; they bypass the typical election in which the absence of symmetry produces some inherent advantage for one of the candidates. Such absence of symmetry could occur, for instance, if candidates are restricted so that they cannot adopt identical strategies or if citizens are predisposed to vote for one candidate. But if we admit such possibilities, we must do so carefully. For example, if the bias of citizens is so strong and unequally distributed that one candidate cannot lose under any circumstance, the choice of strategies—Pareto or otherwise—can be strategically irrelevant. In this essay, we admit the possibility that the election is not symmetric by considering the following alternative to (i) and (ii);

(iii) if a candidate shifts from a non-Pareto optimal pure or risky strategy to its Pareto optimal complement, his plurality increases by at least one vote.[4]

With assumption (iii) we do not use (i), but note that (iii) is a somewhat different and perhaps stronger assumption than (ii). For example, suppose that citizens' choices conform to (ii), that the first candidate proposes to divide one billion dollars equally among all citizens but one, giving this last citizen nothing, and that the second candidate proposes to divide a half billion dollars equally among all citizens and to burn the second half billion. The second candidate's non-Pareto optimal strategy clearly results in his attracting but a single vote. Suppose now that he shifts his strategy to the Pareto optimal complement strategy of dividing the full one billion equally among all citizens. Again, all but one citizen prefers the first candidate, so ϕ^1 is unchanged. Thus, (ii) and (iii) are not consistent. Our interest in (iii), however, is not simply that it permits us to suspend (i), but that it follows from assumptions about voting and abstaining that are used elsewhere in a spatial model of electoral competition.[5] In that model a citizen's probability of voting for a candidate increases, *ceteris paribus*, as the utility the citizen associates with that candidate increases. By shifting from a non-Pareto optimal strategy to its Pareto optimal complement, the candidate increases the utility some citizens associate with him without diminishing the utility of others. Thus, if the assumption of the model is correct, the candidate's plurality increases. Hence (iii) has the virtue of being implied by an assumption about voting from at least one model of electoral competition.

Turning finally to the definition of the outcome of an election, it is evident that candidates should find equilibrium strategies attractive. Assuming then that candidates adopt these strategies, we define the *outcome* of an election as the particular equilibrium strategy that the victorious candidate adopts. (If the election is a tie we let the outcome be either candidate's strategy.) Of course, we do not yet know whether this outcome corresponds to a pure, risky, or mixed strategy, and what this outcome means in terms of the state of society. Suppose, first, that the outcome is a pure strategy. The state of society rendered by the election, then, is this pure strategy. If the outcome corresponds to a risky strategy, the state of society rendered by the election can be defined to be either $P^*(\theta)$—the probability distribution that corresponds to the risky strategy—or the particular θ

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[3] An implicit third assumption is that if a citizen is indifferent between the candidates, he either abstains from voting or chooses randomly by tossing a fair coin.

[4] If S is a non-Pareto optimal strategy (i.e., a proposed non-Pareto optimal state of society or probability distribution over states of society) then S′ is a Pareto optimal complement to S if (1) the utility or expected utility of S′ is greater than or equal to the utility or expected utility of S for all citizens, (2), the utility or expected utility of S′ is strictly greater than the utility or expected utility of S for at least one citizen, and (3), S′ is Pareto optimal. We can readily prove now that for either (iii) or (ii) in conjunction with our other assumptions, if S′ is put against S with the majority voting procedure in use, S′ defeats S. (Parenthetically, we must note that while we do not impose (i) with (iii), we assume throughout this analysis that if a candidate can adopt S, he can adopt at least one Pareto optimal complement to S.)

[5] Melvin J. Hinich, John O. Ledyard, and Peter C. Ordeshook, "Non-Voting and the Existence of Equilibrium Under Majority Rule," *Journal of Economic Theory* (forthcoming); "A Theory of Electoral Equilibrium: A Spatial Analysis Based on the Theory of Games," (Carnegie-Mellon University, SUPA working paper, 1970), and; Melvin J. Hinich and Peter C. Ordeshook, "Transitive Social Preference and Majority Rule Equilibrium with Separable Probabilistic Choice Functions" (Carnegie-Mellon University, SUPA working paper, 1971).

which the victorious candidate selects according to $P^*(\theta)$ once elected. Finally, if the outcome corresponds to a mixed strategy, say $p^*(\theta)$, the state of society rendered by the election is the particular θ which the victorious candidate selects before the balloting.

We can demonstrate now that Shubik's conclusion that the election he models is not Pareto optimal involves a confusion between risky and mixed strategies. Shubik considers an election in which candidates can select either pure or mixed strategies—but not risky ones—and he demonstrates that a mixed strategy is an equilibrium in his example. His argument that this mixed strategy is not Pareto optimal assumes, however, that mixed strategies and risky strategies are equivalent. That is, he assumes that the electoral outcome is $p^*(\theta)$, so that if voters are risk-averse, then for each voter the expected value of $p^*(\theta)$ is less than the value of the state of society rendered by a market.[6] But the correct interpretation of a mixed strategy requires that the state of society rendered in this example be a pure strategy. That is, voters are never confronted with strategies involving probabilities; the victorious candidate wins with a pure strategy which is selected according to the minimax principle *before* the balloting. Thus, we cannot employ this analysis as a counterexample to Pareto optimality in an election. Instead, we must return to the possible kinds of electoral outcomes and examine their properties separately.

For the simplest case—the case in which a pure strategy is the equilibrium—our conclusion is obvious and well known: An election yields a Pareto optimal state of society. To see this, let θ^* be the equilibrium pure strategy S_1^*, but suppose that it is not Pareto optimal. That is, assume that some other strategy θ' exists such that if we substitute θ' for θ^* as the prevailing state of society, the welfare of some citizens is increased but no citizen's welfare is decreased (i.e., θ' is a Pareto optimal complement to θ^*). It follows immediately from (iii), now, that a shift from θ^* to θ' increases candidate 1's expected plurality, which is a violation of expression (2), i.e., $\phi^1(\theta', S_2^*) > \phi^1(\theta^* = S_1^*, S_2^*)$. Suppose, alternatively, that (i) and (ii) hold. Then if θ^* is an equilibrium strategy for candidate 1, it is also an equilibrium strategy for candidate 2; but if θ' is a Pareto optimal complement to θ^*, θ' violates (2) because it defeats θ^* in a majority vote, i.e., $\phi^1(\theta', \theta^*) > \phi^1(\theta^*, \theta^*) = 0$. Hence, θ^* must be Pareto optimal.

By repeating this argument we can show that if the electoral outcome is the risky strategy $P^*(\theta)$ and if we conceptualize the state of society rendered by the election as $P^*(\theta)$, the election is Pareto optimal. Suppose, however, that we conceptualize the state of society as that θ which the victorious candidate selects after the balloting according to the rule $P^*(\theta)$. The question then becomes: Is $P^*(\theta)$ nonzero for any non-Pareto optimal θ (i.e., might a candidate adopt a non-Pareto optimal strategy after the balloting)? To show that for $P^*(\theta)$ to be an equilibrium strategy it must equal zero for all such non-optimal strategies we denote the candidates' admissable pure strategies by θ_i, $i = 1, 2, \cdots$, and the utility a citizen associates with θ_i by $U(\theta_i)$.[7] Assume now that some θ_i, say θ', is not Pareto optimal and that $P^*(\theta') > 0$. Assume also that θ'' is a Pareto optimal complement to θ', which is to say that,

$$U(\theta'') \geq U(\theta') \tag{3}$$

for all citizens, and where strict inequality holds for at least one citizen. Consider now the following strategy as an alternative to $P^*(\theta)$:

$$\begin{aligned} P'(\theta_i) &= P^*(\theta_i) \quad \text{if } \theta_i \neq \theta' \text{ or } \theta'' \\ P'(\theta') &= 0 \\ P'(\theta'') &= P^*(\theta'') + P^*(\theta') \end{aligned} \tag{4}$$

Stated differently, $P'(\theta)$ is identical to $P^*(\theta)$ except that it reduces the probability of the non-optimal strategy to zero, and it increases the probability of the Pareto optimal complement strategy by an equivalent amount. Clearly, then, from (3) and (4), $\Sigma_i P'(\theta_i)U(\theta_i) \geq \Sigma_i P^*(\theta_i)U(\theta_i)$ for all citizens, and strict inequality holds for those citizens who prefer θ'' to θ'. Hence, $P^*(\theta)$ is not a Pareto optimal risky strategy, so that, as before, if assumption (iii) holds, we can violate expression (2) with the Pareto optimal complement to $P^*(\theta)$, while with (i) and (ii), we can violate this expression simply by letting $S_1 = P'(\theta)$ and $S_2^* = P^*(\theta)$. Thus,

> *if the state of society rendered by an election is that θ which the victorious candidate selects after the balloting according to $P^*(\theta)$, only Pareto optimal states are selected.*

The final possibility is that some mixed strategy, $p^*(\theta)$, is an equilibrium. The question, then, becomes: Do equilibrium mixed strategies assign a zero probability to choosing a non-Pareto optimal state of society?[8] To answer this question, assume that there are n available pure strategies denoted θ_i, $i=1, \cdots, n$, and let $p^*(\theta)$ be the first candidate's equilibrium mixed strategy.[9] Suppose now that θ' is some admissible non-Pareto optimal strategy, that $p^*(\theta')>0$, and that the Pareto optimal compliment to θ' is θ''. Consider the strategy $p_0(\theta)$,

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[6] Shubik suggests in his interpretation of a mixed strategy that "we may view the mixed strategy as being a 'degree of belief' in the mind of the voter" (p. 180). Note, however, that if voters are risk averse, there is no incentive for the candidates to have the electorate interpret strategies as risky. And it is with risk averse voters that Shubik attempts to construct a counter-example to an efficient election.

[7] Assuming that there are only a finite number of admissable strategies rather than an infinite number (e.g., θ is a continuous variable) simplifies notation but does not affect our conclusion.

$$(5)\qquad \begin{aligned} p_0(\theta_i) &= p^*(\theta_i) \qquad \text{if } \theta_i \neq \theta' \quad \text{or} \quad \theta'' \\ p_0(\theta') &= 0 \\ p_0(\theta'') &= p^*(\theta'') + p^*(\theta') \end{aligned}$$

We can show now that,

$$(6)\qquad \phi^1(p_0, S_2^*) > \phi^1(p^*, S_2^*)$$

which is to say that (5) violates (2)—p^* cannot be an equilibrium strategy.

We prove (6) by first restating it (letting q^* denote candidate 2's equilibrium strategy).[10]

$$(6')\qquad \begin{aligned} &\sum_i \sum_j p^*(\theta_i)q^*(\theta_j)\phi^1(\theta_i, \theta_j) \\ &\qquad < \sum_i \sum_j p_0(\theta_i)q^*(\theta_j)\phi^1(\theta_i, \theta_j) \end{aligned}$$

Consider the left side of this expression. Suppose that whenever a term with $\phi^1(\theta', \theta_j)$ appears, we substitute $\phi^1(\theta'', \theta_j)$ for $\phi^1(\theta', \theta_j)$. It is readily verified that this substitution simply yields the summation to the right of the inequality. The inequality is satisfied, then, if

$$(7)\qquad \begin{aligned} &\phi^1(\theta'', \theta_j) \geq \phi^1(\theta', \theta_j) \qquad \text{for all } j, \text{ and} \\ &\phi^1(\theta'', \theta_j) > \phi^1(\theta', \theta_j) \qquad \text{for at least} \\ &\text{one } j \text{ in which } g^*(\theta_j) > 0. \end{aligned}$$

Observe now that the second and strict inequality in (7) is simply a restatement of (iii), in which case (6') holds. For the case of a symmetric election—assumptions (i) and (ii)—the first inequality in (7) follows directly from (ii): since θ'' is the Pareto optimal complement to θ', at least as many people prefer θ'' to θ_j as prefer θ' to θ_j. Furthermore, if the election is symmetric we can let $q^*=p^*$, so that $q^*(\theta')>0$. The second condition in (7) is satisfied, then, for $\theta_j=\theta'$, i.e., $\phi^1(\theta'', \theta') > \phi^1(\theta', \theta')=0$. Thus, p_0 violates (2), which is to say that

> *if the electoral outcome is a mixed strategy, the victorious candidate adopts a Pareto optimal pure strategy before the balloting, which is to say that the election yields a Pareto optimal state of society.*

This completes our formal analysis: Whatever the electoral outcome—a pure strategy, a risky strategy, or a mixed strategy—the election renders a Pareto optimal state of society. Of course, if we wish to use this analysis to compare the efficiency of an election with that of a market, someone might object with the argument that a highly abstract description of elections is used to demonstrate their Pareto optimal properties. This objection can be countered, however, with the argument that this abstract description is no more or less real than the abstraction that is used to demonstrate the Pareto optimal properties of markets. The important point is that an ideal description of electoral competition can be constructed which is no less real than the description of perfectly competitive markets and in which the forces for Pareto optimality are evident.

A caveat to this conclusion is afforded now by the observation that thus far we have considered only a single election. Suppose, however, that citizens are evaluating a sequence of one or more identical future elections and market allocations—as they might in the debate over a constitution. Suppose also that a mixed strategy is the equilibrium in each of these elections. Hence, citizens cannot know what policies future candidates will offer because these policies are selected in each election probabilistically.[11] Thus, while the outcome of each election is a Pareto optimal pure strategy, we might interpret the state of society—the state implied by the adoption of a constituion which establishes elections as the mechanism of social choice—as the risky mix of pure strategies rendered by the sequence. That is, the state of society becomes $(p^1(\theta), p^2(\theta), \cdots, p^m(\theta))$, where $p^j(\theta)$ is the equilibrium strategy of the jth election. Each citizen's expected utility, then, is

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[8] In "A Two-Party System, General Equilibrium and the Voter's Paradox," *Zeitschrift für Nationalökonomie*, 28 (1968), 348-349, Shubik concludes, as we do, that the answer to this question for symmetric games is no. He does not, however, provide a general proof of his assertion.

[9] Assuming that θ is finite does not affect our analysis, except that it guarantees that equilibrium strategies exist. If, however, θ is infinite or uncountable, some additional mathematical constraints must be imposed on the candidates' payoff functions before the existence of strategies that satisfy (2) is guaranteed. (See, for example, Guillermo Owen, *Game Theory* [Philadelphia: W. B. Saunders, 1968], pp. 110–112). Since little can be said about the properties of the strategies candidates adopt if equilibria do not exist, we assume that they do exist.

[10] Our notation implies that the second candidate's equilibrium strategy is not a risky strategy. This notation is adopted for convenience, however, and our results follow if we substitute a risky strategy for q^*.

[11] We might, of course, assume simply that for future elections, citizens do not know the particular pure strategies candidates will adopt. Hence, instead of assuming that pure strategies are selected according to the minimax criterion, we could define any probability function, say f, over θ. To conduct any analysis, however, we must say something about f, and so we let f be the minimax function.

$$\sum_{j=1}^{m} \sum_{i=1}^{n} p^j(\theta_i) U(\theta_i) \tag{8}$$

If every election is identical, $p^j(\theta) = p^i(\theta) = p(\theta)$, and (8) can be rewritten,

$$m \sum_{i=1}^{n} p(\theta_i) U(\theta_i) \tag{9}$$

which is simply m times the expected value of the mixed strategy equilibrium. Whether or not this is the interpretation of mixed strategies that Shubik intends we do not know, but if citizens are risk averse, (9) can be less than the utility of the market allocation.[12]

If the electorate consists of risk averse citizens, then, we detect a conflict between two perspectives for evaluating elections normatively. If we ask the question "Are competitive elections Pareto optimal?" our answer is "Yes." If, however, we ask the question "For some forthcoming sequence of one or more social decisions, do citizens prefer elections to markets?" our answer is "Possibly no."

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[12] The expected utility denoted by expression (9) is less than the utility of the market allocation in Shubik's example. If public goods rather than private goods are being allocated, however, markets generally, are not Pareto optimal, in which case we cannot offer any conjectures as to the relative value of (9)—even if citizens are risk averse. Also, it should be emphasized that this analysis assumes that mixed strategies are in equilibrium. If pure or risky strategies are in equilibrium, the corresponding value of (9) will equal or exceed the utility of a market allocation. The extent to which pure or risky strategies are minimax in electoral competition, and the extent to which the approximate nature of these strategies is knowable, then, is an important variable in any normative analysis.

Comment on Ordeshook, "Pareto Optimality in Electoral Competition"

MARTIN SHUBIK
Yale University

The confusion in this paper appears to be caused by the author's failure to appreciate fully a lengthy footnote (footnote 5) in my article. In this note a distinction was made between announcing or failing to announce a mixed strategy. The results from either of these approaches will be Pareto optimal *ex post* but not *ex ante*. Thus, if a price system works (which it usually does not), the citizens *ex ante* should prefer it to the vote unless they are risk positive.

My paper had the narrow goal of pointing out that no attempt had been made to compare the price system and the voting mechanism in the case where the price system is efficient. It then showed that given risk aversion, it was possible that society would prefer the price system. The weaknesses in the article were in the model—particularly in specifying how to embed it in a dynamic context and how to give a mixed strategy (concealed or declared) a more plausible interpretation. My model was adequate for the type of counter-example I was constructing. I would have been happier had the author "bought" far less of my model and tackled the problem of improving its sophistication for a more constructive purpose.

Rejoinder to Shubik's "Comment"

PETER C. ORDESHOOK
Carnegie-Mellon University

First, Shubik's *comment* distinguishes between *ex ante* and *ex post* judgments about markets and elections. He claims that given his assumptions, both markets and elections are efficient *ex post,* though elections may not be efficient *ex ante*. I agree insofar as in my last paragraph I state, "If . . . we ask . . . 'For some forthcoming sequence of one or more social decisions, do citizens prefer elections to markets?' our answer is 'Possibly no.' "

But Shubik's *essay* contains no distinction between *ex ante* and *ex post* judgments. Shubik begins, "it is demonstrated that if all the conditions for the existence of a competitive equilibrium are satisfied, then simple majority voting to determine the distribution of goods may be less efficient than a price system" (page 179). Shubik notes later, "if they [voters] are risk-neutral or risk-averse, then, whenever the 'political noncooperative game' has no pure strategy solution, the result will not be Pareto optimal" (page 180). These two statements indicate that Shubik's judgment is *ex post* and not *ex ante,* and *ex post* he is wrong.

Second, to complicate matters further, Shubik fails to distinguish correctly between mixed and risky strategies by the dictates of these concepts' theoretical definitions. I show that if these concepts are correctly interpreted, then a Pareto efficient allocation occurs no matter what form the candidates' electoral strategies take. Manifestly, an *ex ante* comparison of markets and elections by the Pareto efficient criterion is meaningless, as *ex post* they are both Pareto efficient.

Finally, Shubik's wish that I "had bought far less of . . . [his] model and tackled the problem of improving its sophistication for a more constructive purpose," is interesting. Shubik's "model" is really little more than an example from the spatial theory of electoral competition. Indeed, the results I prove are true for any number of voters, for any number of issues, and for varying probabilities of voting. Shubik's example has three voters, three issues (goods), and no abstentions.

COMMUNICATIONS

To the Editor:

Harlan Hahn's article on Vietnam referenda (December, 1970) deals with questions of great interest and importance for American politics. That the results are basically consistent with those of studies relying on survey data is encouraging.

However, certain technical questions concerning the analysis arise. First, the text suggests that for each city a multiple regression was performed using 12 independent variables. Thus it would appear that for Beverly Hills the number of independent variables exceeded the number of observations (N=5). Ordinary multiple regression breaks down under this situation, so that either Hahn has used far fewer than 12 variables or the multiple R for Beverly Hills is meaningless. (A demonstration of this, largely due to John Ferejohn, is appended.) Interestingly, if Beverly Hills is ignored, the multiple R's vary inversely with the number of observations, which suggests that the differences may have more to do with degrees of freedom than with true explanatory power of variables. Hahn's note 43 suggests he is aware of some of the problems here: he limits himself to first-order partials because of degrees of freedom, but his regressions in effect calculate much higher order partials.

The second question concerns the first-order partials calculated. They are used to determine, in effect, the relative importance of median income and the other variables in determining voter preferences. What disturbs me is that the intercorrelations of the explanatory variables are not presented. The raw data unfortunately are not immediately available to me, so I am left with only the suspicion that median income is highly correlated with at least median education, per cent white collar, and median monthly rent. To the degree that this is so, the meaning of the partials is questionable. The reasons for this are discussed lucidly in James J. Noell's letter in the December issue, so I won't repeat them here. I should point out that the finding concerning percentage of men between 35 and 60 years of age seems to rest on firmer ground than do the other results based on partials.

David Seidman
Congressional Fellow

Appendix

If

$$Y = XB + \epsilon$$

where

$$Y \text{ is } T \times 1$$

$$X \text{ is } T \times K$$

$$B \text{ is } K \times 1$$

$$\epsilon \text{ is } T \times 1$$

The least-squares estimate is

$$\hat{B} = (X'X)^{-1}X'Y$$

Now, for any two matrices conformable for multiplication

$$\text{rank } (AB) \leq \min \left[\text{rank } (A), \text{ rank } (B)\right].$$

(See, e.g., J. Johnston, *Econometric Methods.* [New York: McGraw-Hill, 1963], p. 92.) Therefore,

$$\text{rank } (X'X) \leq \text{rank } (X) \leq \min (T, K)$$

In this case,

$$\text{rank } (X'X) \leq 5$$

But order $(X'X) = K = 12$ (or 13, if a constant term is estimated). Therefore, rank $(X'X) <$ order$(X'X)$ and the determinant of $(X'X)$ vanishes. This means that $(X'X)^{-1}$ does not exist, and therefore the least squares procedure cannot be used.

There is another way to consider the matter. Starting from the same equation, if $K > T$, a set of T columns of X span a t dimensional space which contains Y. Therefore,

$$Y = \sum_{i \in I} \alpha_i X_i \quad \text{exactly}$$

where $I = \{i_1, i_2, \cdots i_T\}$ a set of indices such that $(X_{i_1}, \cdots X_{i_T})$ span the T-dimensional space. This linear combination of X_i's is not unique. In fact there are infinitely many such linear combinations of the X_i's. Therefore, but for the fact that the computational procedure breaks down, with $T = 5$ and $K > 5$ one should be able to achieve $r^2 = 1.0$ whatever the "real" relationship between the dependent and the independent variables.

To the Editor:

Since Mr. Seidman's comments do not question the central conclusions of this study, I will attempt to limit both the length of this letter and the defensive oppugnancy that usually characterizes this type of reply.

The point concerning the computation of the multiple regression for Beverly Hills is, of course, obvious and well taken. The data regarding Beverly Hills were inserted in the article simply for comparative purposes. It is also well known that the distribution of the coefficient of multiple correlation in regression analysis is not independent of degrees of freedom; hence, the slight observed variations in the multiple R for each city are to be expected. Nonetheless, the statistical significance of the coefficients (which can easily be calculated from the information available) also is based upon degrees of freedom, and no claim is made that the multiple R for Beverly Hills is either statistically significant or important. Moreover, the results of the multiple regression analysis reveal striking similarities rather than major differences between communities. The high correlations in separate cities, therefore, seem to provide ample testimony for the main finding of this phase of the research, which is the strength of the association between demographic variables and voting on Vietnam policy.

The second issue regarding the use of partial correlations also seems to be based upon a minor misinterpretation of the study. The partial correlations are not employed to weigh the "relative importance" of each of the measures individually, but they are utilized to compare and to contrast the impact of age and socioeconomic characteristics upon the vote. Since Mr. Seidman notes that the findings concerning generational differences may "rest on firmer ground," it does not appear that he would quarrel with the actual objectives of this analysis.

Perhaps most importantly, the major findings of this study concerning the inverse association between increasing socioeconomic status and opposition to the Vietnam war remain unchallenged. In fact, the questions raised by Mr. Seidman about the statistics in the article seem to reflect a slight misunderstanding regarding the theoretical purpose of their use. I hope that this response will be helpful in resolving his queries.

HARLAN HAHN

TO THE EDITOR:

It appears the desire of Professors Taylor and Herman ("Party Systems and Government Stability," this *Review*, LXV, 1 [March, 1971], 28–37) for categorization and cross-national comparison has forced them into several theoretical and methodological errors regarding their index of government stability.

The dependent variable is based on a simplified, partial, monomeasure of stability. The very elusive and complex concept of stability is reduced to government longevity and stubbornness: the individual cabinet which can hold out the longest is seen as the most "stable." This may measure *duration* but the term *stability* should not be employed. But even if one allows longevity to be employed for stability, and one must for it is really a definitional decision, reliance upon Jean Blondel's view of government duration totally ignores the very real and non-theoretical types of government change which exist in liberal democracies. Cabinets change for a variety of reasons: some changes *do not* present evidence of instability (elections, an additional party joins the coalition, the accidental death of the prime minister—there have been four accidental deaths in the time span employed by the authors and two since January 1, 1969) while other types of change *do* present evidence of less stability (loss of a vote of confidence, a party withdrawal from the coalition, the assassination of the prime minister). By reducing stability to mere longevity, one is just unable to differentiate among these types.

The above comments lead to a further criticism: each and every change is regarded as equal in the sense that stability is lessened. This approach equates a change resulting from an election to one resulting from the assassination of the prime minister. I feel there is a difference in the "stability" levels for both the society and government when elite composition is determined by the eufunctional process of democracy (elections) rather than by assassination. A change due to a loss of a vote of confidence is equated to one resulting from the accidental (perhaps even "neutral") death of the prime minister. I maintain one is in error to force these types into the same category. The authors report that their data are from *Keesing's Contemporary Archives*. One should attempt to sort out the different types of change, especially since *Keesing's* contains all the relevant data.

This desire for a neat category also ignores the French phenomenon of 1958. The structural change in France is only another government change and is equated to any change resulting from a scheduled election. Do the authors really intend to equate the 1964 British election (where Labour ousted the Conservatives) to the change from the Fourth to the Fifth Republic in terms of stability? The entire index is suggestive of Procrustes.

In addition to the above general comments, there are several specific problems involved with the operationalization of the longevity approach which the authors have either overlooked or should have made explicit. How does one deal with cabinets which have resigned and then re-formed without an election but with the same personnel? According to Blondel (and Taylor-Herman), the re-formed governments is not *new* or *different* for it relies upon the support of the same parties and has the same prime minister. I would maintain that the re-formed government *is* new and different for the obvious reason that there is a formal resignation of the old and formal investiture of the new. I have been able to identify 17 such changes for the countries reviewed by the authors: Israel and Italy—4; Austria, Belgium, and France—2; Finland, Luxembourg, and the Netherlands—1.

Another problem which should be made explicit arises when political parties leave their members free to join or not join a particular administration. The representation in the cabinet is done on an individual basis, the parent party is neutral, and if the individual minister resigns, does one posit a *new* government? This was the case with the first government of the Fifth Republic: the Radicals left their members free to join Debré's cabinet but the Radical minister, Jean Berthouin, subsequently resigned in May, 1959. There is also the obverse situation: the case of a party being represented in the cabinet and then withdrawing en masse except for individuals remaining without the parent party's approval. This existed in France when the MRP withdrew from the government in June, 1954, but individual MRP personalities continued in the cabinet until February, 1955. Taylor and Herman give no information on how these problems were resolved.

Given the inadequacies of the authors' index as a measure of stability and longevity, one must interpret with caution the results of the correlation analysis reported on pp. 30–37. One example can be seen on p. 30. It is reported that, between government stability and the number of parties holding seats in the parliament, r = −.39. The authors write that "Lijphart's assertion that 'there is no empirical relationship between the number of parties in the system and its stability' seems unwarranted." Why is Lijphart's assertion unwarranted? Professor Lijphart conceives stability to be a multi-faceted societal attribute, systemic stability, and not mere longevity or cabinet duration. This societal attribute is composed of the absence of violence, the absence of structural change, a high degree of legitimacy, *and* cabinet stability (Lijphart, *The Politics of Accommodation* [California], pp. 71–77). Lijphart is correct—there is *no* empirical relationship between the number of parties in the system and the stability of the system. There may be such a relationship when longevity is employed but "the duration of the government in days" is a shallow representation of systematic stability. Lijphart does not employ longevity as the sole measure although it appears that Professors Taylor and Herman are unaware of this.

LEON HURWITZ

Cleveland State University

TO THE EDITOR:

Professor Hurwitz makes two points: (i) "Cabinet duration" is not the same thing as "stability." Our agreement with this is made abundantly clear in the paper itself, where (at p. 29) we say that duration ". . . is, of course, only one special kind of political stability, and may vary independently of other kinds. . . ." (ii) Our measure of "duration" (or, in effect, our definition of a "government") is crude. We agree completely with Hurwitz that there are many senses in which one can speak of a change in government and that the 'newness' of a government is a question of degree. That our results should be interpreted in the light of the fact that we have chosen just one of these senses and have ignored the question of degree is obvious; and such an interpretation is made possible because our definitions are explicit.

MICHAEL TAYLOR
V. M. HERMAN

University of Essex

TO THE EDITOR:

Since my work is constantly referred to in Jerrold Rusk's recent article in the *Review* (December, 1970, pp. 1220–1238), I wish to exercise the right of reply. Before doing so, let me say at the outset that I have greatly enjoyed reading it, and for two reasons. First, Rusk's article makes the indispensable point that the longitudinal study of voting behavior must not lose sight of changes in institutional parameters which vitally affect that behavior. It is a very well done and useful contribution to the field. Second, I am of course gratified that one part of an article of mine, written a number of years ago, should have provided food for such extensive thought and analysis.

The only proper plea to the charge that I failed to incorporate institutional change in my 1965 analysis must be one of guilty. There is no doubt that this omission is a serious deficiency in that presentation. Having already recognized this, I have attempted to follow up with some institutional analysis in mind in my new book, *Critical Elections and the Mainsprings of American Politics* (New York: Norton, 1970).

At the same time, I cannot escape the sensation that Mr. Rusk has talked past the point of my original argument. It should be noted in the first place that the theory of long-term change in voting behavior which I have suggested in both the 1965 and 1970 studies can be qualified but not effectively refuted, so long as further residues of importance continue to exist in space or time after institutional analysis has explained all it can. The assumption seems implicit in the 1967 registration article by Kelley *et al.* ("Registration and Voting: Putting First Things First," *American Political Science Review,* 61 [1967], pp. 359–379), that institutional changes explain all that is worth explaining. This assumption is all but flatly stated in Rusk's article, where he asserts that the behavioral variables I discussed turn out to be of "minuscule" importance (p. 1236). This surely goes much too far in the opposite direction, as we shall see below. So long as significant elements of behavior which cannot be plausibly explained by the direct intervention of institutional variables continue to exist—and they do—such overstatement of the institutionalist hypothesis "puts first things first" at the cost of excluding politics from the study of political phenomena. To take one example of a spatial residue, it can be demonstrated in at least one important state that declines in turnout between 1900 and 1918 were at least as great in areas not covered by a 1906 personal-registration statute as in urban areas whose populations were subject to this new requirement; and not less than one-fifth of the state's population was involved.

At least as strong a case can be made with regard to time sequences and the effect of the Australian ballot reform. I take it as axiomatic that the pre-1888 absence of split-ticket voting was largely an artifact of the unofficial party ticket, and that the official ballot substituted for it more or less facilitated such voting, depending on its specific structure in a given state. But my original argument, as well as that developed in my 1970 study, is concerned with significant behavioral change whose full dimensions mature some years *after* Rusk's cut-off date of 1908. Moreover, if it can be shown that the most quantitatively significant change in split-ticket voting occurs years *after* the introduction of the Australian ballot, then we would seem to have a variable which makes possible but does not cause or create a certain behavioral pattern. My argument had to do with the behavioral consequences of the reorganization—indeed, disorganization—of American electoral politics which progressively unfolded during the 1896–1932 alignment era. It would be more likely to be effectively demolished if a showing could be made that behavioral change in the wake of the ballot reform was more or less immediate and that subsequent changes were at least of no greater quantitative magnitude than those established in the 1890–1908 period. Such a showing, however, cannot be made. Before turning to a more detailed exposition, I would note that the "split-ticket voting" index which I used in 1965 now seems to me an inadequate substitute for a much more straightforward procedure. The latter is the generation of a mean percentage for each party and a corresponding standard deviation for all offices within a single election for which the state as a whole is the constituency, followed by a reiteration of this procedure for each election in a time series. In other words we can take advantage of the long ballot in the states where it exists, and from it derive a series of data points for each election. Basic data are presented in Appendix Tables I-V of my *Critical Elections* book for five states—Indiana, Massachusetts, Michigan, Rhode Island, and Wisconsin. The discussion which follows is primarily based on it.

It would be reasonable to anticipate that the introduction of an institutional change should lead to some systematic change in voting behavior which would produce effects extending over a considerable period of time. A useful device for measuring this, it seems to me, is the use of the t-test as a longitudinally descriptive measure which permits identification of the date at which system change appears to have occurred. Assuming stability before and after change, a time span of about a decade—at annual or biennial intervals—appears appropriate for each group of observations to be processed. Thus we do find in Massachusetts, for example, a quite systematic change with a midpoint identical to the date of introduction of the Australian ballot; but this change has to do not with standard deviations measuring split-ticket voting, but with roll-off, or the percentage of incomplete ballots. The mean roll-off for 1880–1888 is 1.0%, and for the 1889–1897 period 6.1%, with $t = 5.03$. Even if we do not apply standard tests of significance here, the cutting point is clear—particularly in view of the fact that no other partitioning of the data by any decade set yields a t which is anywhere near as large.

When we apply a set of iterated t-tests for the five states included in this analysis, we can identify "high" midpoint years for the era of ballot reform, plus subsequent "highs" indicat-

Table 1. Changes in Split-Ticket Voting in Three Time Periods

State	Ballot Era Period	S.D.	*t*	Later Positive Change Period	S.D.	*t*	Later Negative Change Period	S.D.	*t*
Indiana	1882–90	0.14	0.47	1888–96	0.09	1.52	1920–28	1.49	3.50**
	1892–1900	0.11		1898–1904	0.22		1930–38	0.25	
(Midpoint)	(1891)			(1897)			(1929)		
				1912–20	0.23	3.03**			
				1922–30	1.43				
(Midpoint)				(1921)					
Massachusetts	1880–88	0.85	0.97	1895–1903	1.14	4.19**	1922–30	4.28	1.30
	1889–97	1.21		1904–12	3.11		1932–40	3.23	
(Midpoint)	(1889)			(1904)			(1931)		
Michigan	1882–90	0.46	1.94	1894–1902	1.10	2.42*	1922–30	4.59	2.38*
	1892–1900	0.97		1904–12	2.95		1932–40	1.93	
(Midpoint)	(1891)			(1903)			(1931)		
Rhode Island	1880–88	1.61	1.29	1887–96	0.71	3.60**	1910–18	3.41	2.95**
	1890–98	0.86		1897–1904	2.14		1920–28	1.28	
(Midpoint)	(1889)			(1897)			(1919)		
				1893–1901	1.13	3.92**			
				1902–10	2.52				
(Midpoint)				(1902)					
Wisconsin	1881–88	0.91	1.06	1902–10	1.08	2.57*	1932–40	9.17	2.48*
	1890–98	0.38		1912–20	3.88		1942–50	3.92	
(Midpoint)	(1889)			(1911)			(1941)		

* $p \leq .025$, one-tailed test
** $p \leq .001$, one-tailed test
All others not significant at or below .025

ing sudden increases to more or less durable levels, and still later—mostly during the New Deal era—further "highs" in the series of *t* values associated with *declines* in standard deviations.

Now this array presents some anomalies in terms of Rusk's argument. I leave aside the curiosity that Indiana, Rhode Island, and Wisconsin show *declines* in the magnitude of standard deviations during the ballot-era election sequences showing the highest *t*-values. Despite this curiosity, I remain persuaded that Rusk's case for the existence of an influence is strong. The time dimension is a different matter. In all five cases, a much stronger case can be made for "system change" in the second and third columns of the table above than in the first. Taking the midpoint years of highest positive change as a benchmark, the first system transformation—toward positive increases in the standard-deviation measure of split-ticket voting—occurs between 12 and 30 years after the ballot-reform era high point in our five states, with a mean of 18 years. This is particularly informative in the case of the office-block state *par excellence,* Massachusetts. What is really remarkable is that the standard deviations tend to remain extremely small until 15 years after the appearance of the Australian ballot. The only significant *immediate* effect of this reform, as we have said, was the sharp increase to new stable levels in the percentage of incomplete ballots—hence a *t* of 5.03 for roll-off, compared with one of only 0.97 for standard deviations in the same time sequence. In view of the exceptionally antipartisan form which the Massachusetts official ballot took, and which it has since retained, voting a straight ticket in the 1889–1903 period required exceptional partisan persistence on the voter's part. This makes the small standard deviations prior to 1904 all the more remarkable.

The point is that the Australian ballot only facilitated subsequent behavioral change, it did not create it. What happened between the ballot-era period and the time when "system-change" positive increases in split-ticket voting occurred in these states? The answer, obviously enough, is to be found primarily in the critical realignment of 1896 and its consequences for parties thereafter. Similarly with the negative change falling still later: it is associated in each case with the development of more or less contemporary intensities of two-party competition, with the modal point falling during the New Deal Realignment era. That the institutional variable itself loosened up party voting and provided a favorable context for subsequent changes in voting behavior I would not deny. All I have ever argued is that this behavioral change very clearly had a life of its own. That argument seems not to be essentially disturbed by Rusk's findings, for it remains quite independently grounded.

It seems clear enough that the steep rise in the standard deviation of the partisan vote in and after 1904 is closely related to the *extent to which two-party competition survived the sectional realignment which came into being after 1896.* So also, not surprisingly, are roll-off and turnout. If we place our five states on a continuum of party competition for the decade 1920–1930, the high water mark of the "system of 1896," the relationship emerges quite clearly, particularly when compared with the prereform decade of the 1880's. The array also serves as a useful reminder that all of these variables are metrics of a dynamic system of political action—a system in which both the behavioral and the institutional aspects of political change play a major role. It may also be added that the subsequent decline in standard deviations associated with the restoration of two-party competition, which is common to all five of our states, merely reinforces the behavioral-change side of the controversy.

Perhaps the most crucial deficiency in my original omission of such institutional changes in the 1890–1910 period lies in the failure to integrate these changes into the argument, since they may as much support as qualify it. For changes in electoral law are not politically or sociologically neutral, as we are beginning to see in the study of personal-registration statutes and their effects on participation. These 1890–1910 reforms are not only intervening and partially causal variables, they are also caused variables. This becomes particularly apparent when they are seen together. For my own part, I continue to suppose that the causes of both the reforms and the behavior are probably not profoundly dissimilar from those originally suggested in 1965.

One basic conclusion to be drawn from all this is that a more completely multivariate theory of change in and structure of American voting behavior across time is urgently needed. This should begin at the least with the full presentation of the characteristics of the universe —both behavioral and institutional—which remains to be published. Then and not before are we likely to achieve the desired goal of "putting first things first." In the meantime, we should remain on guard not only against omitting sufficient conditions for the appearance of phenomena, but against confusing sufficient with necessary conditions. The correctives which Kelley and his associates and Rusk have provided are of very great importance and value. But I feel bound to say that no conclusive evidence has yet been presented that my original argument or its 1970 reformulation need be abandoned in any essential respect.

WALTER DEAN BURNHAM
Massachusetts Institute of Technology

TO THE EDITOR:

Professor Burnham's article in the March 1965 *Review*—"The Changing Shape of the American Political Universe"—has been an important piece in stimulating discussion and research in the area of the historical analysis of American electoral behavior. Burnham has performed a major contribution by focusing our attention upon the changing distribution curves across time of five key dependent variables of

Table 2. Partisan Competition and Other Electoral Indices: The 1880's and the 1920's

	1880–1888				1920–1930			
	% Dem.	S.D.	Rolloff	Turnout	% Dem.	S.D.	Rolloff	Turnout
Indiana	50.1	0.11	0.3	89.8	47.2	1.27	3.0	66.1
Rhode Island	42.2	1.61	1.4	38.5	44.6	1.14	1.7	60.1
Massachusetts*	44.0	0.91	1.5	59.5	42.9	3.95	9.8	55.7
Michigan	47.3	0.46	1.0	72.7	28.1	4.25	6.7	42.4
Wisconsin	43.8	0.91	1.1	75.9	19.8	5.81	17.7	47.1

* Office-block ballot; others, party-column.

voting behavior—split-ticket voting, roll-off, turnout, partisan swing, and drop-off. Other scholars—such as V. O. Key and Robert E. Lane—have also noted the changing properties of these variables, but it was Burnham who focused our attention on them as a collective set and, in turn, proposed a theory to explain their changing nature.

Basically, Burnham noted that turnout levels were high, while split-ticket voting, roll-off, drop-off, and partisan swing levels were low up to the 1890's. After that, the reverse became true—turnout started declining, and split-ticket voting and the other voting indices began to rise in value. Burnham believes these changes can be linked to the election of 1896. According to Burnham, a business-Republican elite coalition succeeded in stealing the electoral system from the people as a necessary step in consolidating the industrial revolution, in essentially dictating the revolution's development without having to be subjected to mass pressures and demands. This elite capture of the electoral system was assumed to have led to a major breakdown in party organization and competition in the aftermath of the 1896 election, and, correspondingly, to an alienation of the voter from the political system. Prior to that time, the voter was considered to be firmly entrenched in the system with his strong party loyalty.

Over the last seven years, these basic ideas—quite rightly called a "theory"—have gained considerable currency in the field, so much so that at least one vital point seems to have been overlooked—that they have never been empirically confirmed. The '65 article by Burnham must be essentially viewed as an exercise of laying out the distribution of five dependent variables and then attempting to set forth a theory to explain the systematic changes in them. The theory may be correct, partially or completely, or it may not, but it is obvious that there has been little attempt to operationalize the concepts in the theory as independent variables in order to test empirically their relations to the systematic changes in the dependent variables of split-ticket voting, turnout, etc. One realizes, at the same time, that asking the investigator to do this entails a difficult task. The problems involved in operationalizing and obtaining empirical information on such variables as business elite take-overs of voting systems, breakdowns in party organization, psychological alienation of the voter from the political system, etc., are very apparent. Historical accounts of such matters are often scarce or of dubious quality, and the "psychological" aspects of past political behavior are difficult to probe for as a marked contrast to our contemporary knowledge of such matters when using sample survey techniques.

These same problems again surface when confronted with Burnham's second statement of his theory—his book on *Critical Elections and the Mainsprings of American Politics* (1970). The reason for this lies in the fact that this statement, in all essential respects, is identical to that first proposed in his 1965 *Review* article. Burnham does qualify his argument slightly with the introduction of institutional variables (ballot, registration systems) into the discussion, but basically the attempt is to incorporate such new considerations to fit his thesis rather than to revise his central contentions. From reading both statements of his thesis, one senses the confidence Burnham has in its ability to explain the changing characteristics of the electorate. Undoubtedly, important changes were taking place within the "historical electoral universe"—changes that must have some explanation. This author shares Burnham's concern with explaining these changes—the differences between us reside in what we consider to be causing these changes and why.

This author's basic belief is that institutional variables were the important factors prompting the many changes in the electorate around the turn of the century. The introduction and establishment of registration systems could make the vote turnout curve drop. The introduction of the new Australian Ballot system, which was secret and consolidated, replacing the separate party ballots of an earlier day, could have effects in increasing split-ticket voting. Once all parties had to be placed on the same ballot and a secret ballot was used, the potential for split-ticket voting would certainly be improved. The new ballot system, and the voting rules accompanying its introduction, could also cause increases in roll-off, the tendency for the electorate to vote in lesser numbers for lower-placed offices on the ballot. Having to consider all offices on the new consolidated ballot and to vote on them was definitely a more tedious and difficult decision-making task than simply depositing the earlier system's single party ballot in the ballot box, either marked or unmarked. No decision making was required in the latter situation, other than which party's ballot to pick up in the first place.

This essentially was the theory laid out in the author's article in the December 1970 *Review*, stating that institutional variables had a vital and logical role in explaining the changes Burnham had noted in turnout, split-ticket voting, and roll-off. (At the time it was not stated, but connections could also be seen between these variables and the explanation of Burnham's drop-off and partisan swing figures, since the behavior of these two voting indices hewed closely to and would necessarily be explained by the same institutional factors as Burnham's turnout and split-ticket voting indices, respectively.) The next step was to test these ideas empirically, and this was attempted for only that part of the theory dealing with ballot and its relation to the split-ticket voting of that time. This limitation was imposed by the large task of collecting such historical information as well as by the realization that the initial research project had to be of a fairly specific scope. Later, the author and others collected information on registration and other electoral qualification variables in an attempt to relate these to Burnham's turnout figures, the analysis of these materials to appear in future publications.

The results of the ballot analysis confirmed the expectations of the theory—that the introduction of the new ballot system facilitated increases in split-ticket voting in a systematic way that could be empirically verified state-by-state. It was also discovered that, for the time period studied, there was very little, if any, split-ticket voting left over for Burnham's theory to explain after ballot analysis had accounted for what it could in the specific four states of Burnham's concern in his 1965 article.

These results naturally led to the question of the status and relationship of this work to that of Burnham's, a question which prompted this exchange of letters to the editor. There are several different levels on which this can be discussed. A major point, and one which was emphasized earlier, was that neither Burnham nor others have empirically verified his theory, while in the case of ballot, the theory has been operationalized and empirically demonstrated. Burnham, in essence, has confined his discussion to the theoretical level while the treatment of ballot has extended to both the theoretical and empirical levels. Thus, much of Burnham's comments above with regard to his argument being sound in light of the ballot analysis must be seriously qualified—since his theory has not been confirmed. It is difficult, from this perspective, to understand how he can so forcefully maintain that his theory is correct per se, or how he can believe it to be correct and basically unaltered when empirical evidence suggests the contrary.

There is another level on which our two works can be discussed, if only briefly. Outside of the analysis on the ballot variable, the rest of the theory regarding institutional influences on voting behavior still needs to be empirically tested. Work on this score is now underway, but one must admit that any discussion of the impact, for instance, of registration system variables on turnout must remain on the same plane as Burnham's theory, as basically unproven until the empirical work is done. But the central point is clear—that theories have to be tested, not merely proposed, and only in so doing can we state without equivocation that they are valid.

Returning to the main concern of Burnham's remarks—the empirical analysis of the ballot variable specifically—one can sketch out his basic points as follows. The first argument advanced by Burnham is that there are further residues of importance existing in space and time after institutional analysis has explained all that it can. While one can easily agree with such a general claim, one, at the same time, has to qualify this statement in certain respects—and such qualifications tend to weaken the Burnham theory. For example, with regard to the specific time period the author studied, 1876–1908, it was quite clear that ballot could explain almost all of the split-ticket voting in the four states that Burnham had selected for study. If his theory were correct, one would have expected large residues of split-ticket voting to remain, since his theory predicts widespread, not minuscule, changes in voting behavior. However, if Burnham were to extend his analysis beyond four states to the universe of states (as was done in the ballot analysis), it is possible that larger residues of such voting might be discovered. If so, the question would then become one of whether Burnham's theory or some alternate theory of this period could explain these residues.

Another qualification of Burnham's argument comes to mind when one extends the field of focus beyond 1908, the cutoff date of the ballot analysis. Residues of split-ticket voting could exist in later periods, such as the 1920's (which Burnham refers to as the "high water mark of the system of 1896"), but here a "system change" far more basic than the consequences of the 1896 election would probably account for such heightened activity in split-ticket voting—the advent of women's suffrage. Opening of the system to a relatively "politically unsocialized" contingent without strong party ties could provide the flux in the electoral figures of the 1920's which such observers as Burnham have noted. (The logical connection between further declines in turnout in the

1920's and the introduction of women's suffrage is also apparent.) Current survey research still shows traces of such behavior, a behavior that would be much more accentuated in the years immediately following enactment of the Nineteenth Amendment. (For a more cogent statement of this position, see Philip E. Converse, "Change in the American Electorate," unpublished Michigan Survey Research Center paper, 1970.)

A second point that Burnham makes is that the effects of the new ballot, especially the large effects, seem to have come some time after it was introduced in the several states, implying that only a demonstration of large immediate effects could be viewed as substantial evidence in discounting his theory. Before one addresses this charge, however, one must clearly state that the introduction of the Australian Ballot largely antedated the 1896 election which Burnham believes spurred on the many changes in voting behavior described earlier. Almost all of the states had the new ballot before 1896, and the effects it had in these earlier years (before Burnham's theory comes into play) are quite evident in the data. Still, such effects were not "immediately large" for every state, which raises the obvious question—should they have been?

Throughout the ballot article, the author contends that it took some time for people to adjust to the split-ticket potential of the party column version of the new ballot, but relatively little time to adjust to the office-block-format. To the extent that this is true, it would mean that fairly immediate, large split-ticket effects would be registered in office-block states, once the new ballot was enacted, but smaller effects would be observed in the case of party column states. This seems natural given the historical situation at the time and the physical differences between the two types of ballot format. Imagine the historical situation—that the new ballot had been introduced against the backdrop of a strongly partisan culture and its separate party ballots. It would take people some time to adjust to the new freedom provided by the Australian Ballot, the amount of time depending on which format of the ballot was used in their state. The adjustment time needed for the office bloc format would be relatively little, since its basically nonpartisan structure departed radically from the earlier partisan ballot. On the other hand, a longer adjustment time would be required under the party column format, since it resembled in many respects, and probably was meant to perpetuate, the partisan culture of the previous ballot era. The data bear these points out—split-ticket voting rose considerably faster in office-block than in party column ballot states; office-block ballot states, in the beginning, were registering the fairly immediate, "large" split-ticket effects. As time passed, the party column ballot states started to register "bigger" effects as more of the electorate adjusted to the split-ticket potential of this ballot (namely, its features of having both parties on the same ticket and allowing for a secret vote), but the very nature of its partisan format, as well as its ties in the early stages of the new ballot period to the previous one, set an upper limit on the extent to which these states could approach the split-ticket records of the office-block ballot states.

Given this evidence and the theory underlying it, there is no reason to expect the Burnham notion of large split-ticket effects having to occur immediately for every state upon enactment of the new ballot. Such a reading misses the "character" of ballot as a variable and the setting in which it was placed, as well as the considerable discussion and documentation of these points in the ballot article (see the discussion on pp. 1230–1233, point (3) of the article's summary on p. 1235, and the data contained in Table 5 and Figure 3). The fact is that the office-block states do tend to conform to the Burnham notion, but the party column states do not, in the beginning, and there is no reason to expect them to do so.

The workings of ballot as a variable are, however, even more complicated than the "adjustment theory" sketched thus far would indicate. The relative use of the split-ticket potential of the new ballot, in the early days of its adoption, depended not only on the rate at which people adjusted to the type of format used, but also on elections existing which produced a desire to use the split-ticket property of the ballot (a point made on p. 1231 and, more indirectly, on p. 1228 of the ballot article). To some extent, these two events interact, although it is more than plausible that a given format (i.e., party column) could have an inhibiting effect on the degree to which a given election produced desires to split one's ticket. This was already evident in the data alluded to in the discussion above on "adjustment effects"—the degree to which party column states had lower split-ticket figures than office-block ballot states. Still, at the same time, it is definitely acknowledged that given elections producing more split-ticket-prone stimuli are more likely to have greater split-ticket voting than elections associated with less split-ticket-prone stimuli. Ballot in this sense helps to facilitate the implementation of split-ticket desires. This brings us to a crucial question raised by Burnham and best expressed in his statement "... that the Australian Ballot only facilitated subsequent behavioral change, it did not create it."

It is Burnham's contention that ballot was more a facilitative or contributing cause than a direct cause of the movements in the electorate's split-ticket voting behavior. Some of the evidence suggests this; some of it does not. With regard to the latter, it is quite clear that the earlier ballot system was most probably a direct cause in not allowing the electorate to vote a split-ticket. Burnham also, despite his above statement, agrees with ballot being a basic cause in this regard, when he says, "I take it as axiomatic that the pre-1888 absence of split-ticket voting was largely an artifact of the unofficial party ticket. . . ." From the way this ballot system was constituted, it could hardly be otherwise—separate party ballots peddled by party workers in a nonsecret voting setting. Your only choice seemed to be which party's ballot to take; to split your ticket was extremely difficult. Given this situation, it is little wonder that ballot is perceived as a direct cause of voting behavior. As for the new Australian Ballot, the data show that many states experienced increased split-ticket voting just after the new ballot was introduced. Since such results continue to be maintained across time, it is evident that the major shift to the Australian Ballot had some depressing effect on the intensity of partisanship, lending increased amplitude to cross-party voting. Other evidence also suggests the independence of ballot as an explanatory factor—especially the demonstration that different adjustment patterns to its split-ticket potential could be traced to the different types of ballot format in use. Even today, the fact that greater split-ticket voting occurs in office-block than in party column states suggests some independent effect of ballot on voting. (See Angus Campbell and Warren E. Miller, "The Motivational Basis of Straight and Split Ticket Voting," this *Review*, 51 [June 1957], 293–312.)

Obviously, however, the new ballot is not directly causing all split-ticket voting—it quite clearly by its presence also facilitates the expression of split-ticket desires which are independently grounded, desires that were virtually impossible to implement under the earlier party ballot system. Viewing ballot in this light—as a facilitating or contributory cause rather than as a direct cause—in no way substantiates the Burnham theory. The most likely prospect is that ordinary motivating factors—the customary SRC-designated "short-term forces" of issue and candidate appeal—were the impetus for people to split their tickets under the Australian system. The Australian system had loosened up party voting to such an extent as to allow such short-term forces to play a role in determining the vote. In so doing, its function could be seen as a "direct" one as well as a "facilitating" one. Pushing the argument further, the degree to which such short-term forces were translated into split-ticket votes would vary as a function of the adjustment process underlying the new ballot as well as the type of ballot format in use—clearly "direct causal" factors. On the other hand, Burnham contends that the causal factors at work were the anti-party forces emerging from the 1896 election, forces which used ballot merely as an instrument to express their wishes. This, of course, is possible, but the fact that large spurts of split-ticket voting did not occur continuously after the 1896 election calls into question the existence and widespread impact of such forces across this time period. The variable nature of split-ticket voting would more likely call on a theory which states a "high" or "low" split-ticket election would be determined by the salience and intensity of the short-term forces existing in that election year. Of course, the theme of ballot is central throughout—the new ballot had to be introduced in order for people to split their vote, and even a "low" split-ticket year would most usually have a higher split-ticket value in the Australian period than in the pre-Australian period, simply because some people would be using the new ballot's split-ticket feature. When one looks back to the pre-Australian days, one would acknowledge that short-term forces existed then also (although in no degree comparable to Australian days because of the nature of the pre-Australian ballot and its corresponding partisan culture), but the crucial point is that it was extremely difficult to translate such forces into split-ticket votes using the single party ballot system.

The last contention Burnham makes is that ballot as a variable can be incorporated into his theory. To do this, he would have to account for the above arguments, as well as the two possible discrepancies that follow. First, the introduction of the Australian Ballot antedates his theory of the 1896 election and its consequences. Second, although the necessary research has yet to be done, it seems that the forces most instrumental in pushing for the Australian reform were "reformers" and "good government" people and not "business-Republican elites" intent on seizing control of the electoral system. If his theory were thought to be basically compatible with the ballot theory, it would seem that it at least would have to be revised to take account of these points. As it stands, it is difficult to see how Burnham can maintain the belief that the causes of both the ballot reform and split-ticket voting were the forces underlying the election of 1896.

This discussion of Burnham's last point leads us inevitably to a final confrontation of the question that has been front and center throughout this paper—the question of the status and relationship of the institutional and Burnham explanations. It would seem that much of the answer would depend not only on what has been stated above but also on some fairly specific future research efforts. First and foremost, the Burnham theory would have to be operationalized and tested to the extent that it could be. On the institutional side of things, moving past "ballot" as a variable, one must now test for the influence of other institutional factors on Burnham's voting behavior variables—most specifically, ascertaining the relationship of registration and other electoral qualification variables to voting turnout. Also, the effect of women's suffrage on both turnout and split-ticket voting should be analyzed. If the other institutional variables prove to be as rewarding in explanatory power as ballot (and as Kelley *et al.* suggest they would be for registration variables), the author would heavily subscribe to the theory of the interaction of institutional and short-term effects mentioned above as the best explanation of the voting behavior around the turn of the century. If the Burnham theory should be confirmed, this would result in a mesh between the institutional variables and the Burnham theory, provided the latter is modified in certain respects. Until such further evidence is in, however, the author maintains his belief that the data contained in his *Review* article and the points specified above argue strongly for (a) the fact that ballot has a life of its own in affecting the split-ticket process, and, at the same time, (b) that it seriously qualifies the chief arguments in the Burnham theory.

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BOOK REVIEWS

Serious Games. By Clark C. Abt. (New York: The Viking Press, Inc., 1970. Pp. 176. $5.95, cloth; $1.95, paper.)

A Primer on Simulation and Gaming. By Richard F. Barton. (Englewood Cliffs: Prentice-Hall, Inc., 1970. Pp. 239. $8.95 cloth, $4.95 paper.)

Serious Games is a book with a message and is written with almost evangelistic zeal. While conceding that games are not a panacea for all the ills of modern life, Abt argues that they can provide fast and effective relief for many of these ills. To appreciate this remarkable palliative power, Abt invites the reader to view the world through game-colored glasses. In a formal sense, "a game is an *activity* among two or more independent *decision makers* seeking to achieve their *objectives* in some *limiting context*" (p. 6). Since most real-life activities may be conceived in these terms, a game becomes "a way of looking at something, anything" (p. 5). Presumedly, this "way of looking" affords one considerable analytic leverage and fosters a creative yet rigorous type of problem-solving. It also allows one to contrive activities (serious games) designed to simulate social processes which then can be used to communicate knowledge about these processes and to surface new insights regarding them.

A major portion of the book is devoted to the educational value of games. Abt contends that games can assuage many of the problems of contemporary education—problems relating to student motivation, curricular relevance, instructional responsiveness, testing and evaluation, and limited resources. While games allegedly can improve education in general, Abt suggests that they are especially beneficial for students from disadvantaged areas. He argues that games can provide a mode of activity familiar to these students and thus sustain their interests and involvement. Moreover, through such games, disadvantaged students can exhibit important skills and talents that otherwise might go undetected.

Moving beyond the classroom and formal education, Abt finds serious games to be exceptionally valuable tools for planning and problem-solving in government and industry. This value accrues in part through a sort of dialectical process. Games promote a sense of detachment and objectivity, while at the same time engaging the emotions of the participants and fostering an intense involvement with the problems being gamed. Games thus offer unique advantages when used to analyze specific problems, experiment with alternative strategies, and convey substantive information.

While arguing that participation in serious games can be both enjoyable and highly rewarding, Abt also points out that all of the payoffs are not in the playing. The designing of a game can be a potent educational experience and an illuminating analytic exercise.

Throughout the book, Abt documents his arguments with personal anecdotes and an occasional testimonial from a satisfied user. With the exception of a single reference to Boocock and Schild (eds.), *Simulation Games in Learning,* he essentially ignores the large and growing body of research and literature on simulation and gaming. Sprinkled throughout are fairly detailed descriptions of a number of gaming exercises, ranging from simple card and board games to fairly elaborate, computer-assisted simulations. Many of these were designed by Abt Associates, Inc., of which the author is founder and president.

Perhaps the most interesting chapter in the book is devoted to the "cost-effectiveness" of games. While Abt's conclusions are never in doubt, he does raise a number of important questions and issues about the design and use of games. Unfortunately, most of these are dismissed rather glibly. The essential message of both the chapter and the book seems to be: "Let gaming be pervasive, let it flourish in good forms and ill, and the best will emerge. This is no art of the experts, but a universal language common to all cultures, ages and conditions" (pp. 114–115).

While Abt's book is addressed primarily to the question of why do it, Barton's *Primer* concentrates more on how it is done. For Barton, *"simulation is simply the dynamic execution or manipulation of a model of an object system* [i.e., the subject matter of an investigation or learning experience] *for some purpose"* (p. 6). The burden of the book is, in part, to demonstrate that technical expertise is a more important asset in the construction and use of simulations and games than Abt would lead one to believe.

Barton identifies four ways of studying object systems: (1) analysis, (2) man-model simulation, (3) man-computer simulation, and (4) all-computer simulation. The book is organized largely around the last three of these techniques, detailed consideration being given to each in turn. Each chapter is supplemented by a number of study exercises, some relatively simplistic, others demanding more than the reader is likely to be prepared to do on the basis of the text.

After introducing the reader to simple flow diagram conventions, Barton uses a number of relatively simple but illuminating flow charts to illustrate and summarize his arguments. The book develops very slowly and apparently is intended to move progressively from the simple to the more complex. However, the progression is far from uniform and the chapters are uneven in detail and the level of prior sophistication presumed. For example, a person without some training in statistics or probability theory is likely to find parts of the chapter on Monte Carlo techniques exceedingly difficult, if not impossible, to follow.

The continuity of the book also suffers from a fairly lengthy digression on computer systems. Barton's advice on computer etiquette is sound, but the instruction he provides on the jargon of computer systems is of marginal relevance at best and is drawn out unnecessarily. For the uninitiated, this exercise in grammar is likely to be largely meaningless. For the person with some computer experience, it serves no real purpose.

In his chapter on simulation languages, Barton offers a brief but useful comparison of three simulation languages: GPSS, SIMSCRIPT, and DYNAMO. Before getting to these comparisons, however, he leads the reader through an excessively long discussion (replete with examples) of internal and external machine languages, assembly languages, and user-oriented (source) languages and through an annotated listing of several higher level languages, most of which are not well known nor commonly available.

The book is generally strong on the mechanics of constructing and implementing simulations but is less helpful on matters relating to structural desiderata and design criteria (perhaps understandably, since these matters are heavily dependent upon the purpose for which the simulation is to be used). Barton's advice regarding programming and computer usage is particularly well-taken, and he offers a number of valuable rules of thumb for constructing simulations. The force of his insights are sometimes lost, however, amidst a plethora of irrelevant information and a tendency to belabor the obvious. Although he is emphatic in asserting that simulation should not be used when analysis will do, the force of his argument is diluted by an unfortunate choice of a simple penny-matching game with which to illustrate the various techniques for studying "object systems." While Barton does provide a limited bibliography, he, like Abt, does not attempt even a cursory survey of the literature. His final chapter "A Survey of Simulation Applications" does not contain a single citation. This is somewhat ironical in that Barton makes a strong argument for the importance of documentation when constructing simulations. The same apparently does not hold when writing books.

While some simulation and gaming practitioners may find succor in Abt's uncritical exposition of the faith or enjoy some of Barton's inside dope, I suspect that most will find both of these books disappointing introductions to the subject. Although the authors of both books obviously have had extensive experience with their subject matter, more thorough attention to the literature would have added much, not least of which is a fuller and more useful perspective for their focal concerns.

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Embattled Reason, Essays on Social Knowledge. By Reinhard Bendix. (New York: Oxford University Press, 1970. Pp. ix, 395. $9.75.)

This is a collection of essays published over a twenty-year period by a scholar of wide erudition and profound sensitivity to our current situation as well as our past. It is, in his words, "a piece of intellectual autobiography." Three chapters deal with the "Conditions of Knowledge," three with "Theoretical Perspective," and the remaining six with "Studies of Modernization."

The volume is so rich in important argument and powerful propositions that it is hard to know where to begin. There are certain major themes, however, that bind the wide range of subjects. The book begins with reflections upon the precarious state of dispassionate inquiry in the modern University, with the major emphasis upon social inquiry. Bendix remarks again and again the paradox in our trust in reason, on the one hand, and our cultural despair; our belief in the inquiry of the scholar and our distrust of the average man's prudence which such inquiry often inspires. In "Social Science and the Image of Man," he notes the applicability of Zeno's paradox to the social scientist himself, asking how Marx's thought could be exempt from Marx's strictures on ideology, or what dark complex of childhood experiences lay at the roots of Freud's analysis of unreason, and thereby "explained" it.

His general counterproposition is a condemnation of reductionism, whether that of Durkheim on one hand, or the fashionable "sociology of science" argument of Kuhn on the other.

> Durkheim and Freud are heirs of the Enlightenment. They came to see, though perhaps not explicitly, that a completely reductionist view of human nature is incompatible with sociology and psychoanalysis as intellectual disciplines. For such disciplines cannot remain free of contradiction, if steps forward in the analysis of man are steps backward from the belief in knowledge "for the benefit and use of life" (p. 11).

The image of man as a being whose behavior can be reduced to a few overwhelming factors leaves a completely determined, therefore impotent, homunculus in place of the human being. "This image of man . . . contributes to the antirationalist tendencies of our time" (p. 12). Thus to the Marxist, "universities are institutions of learning only superficially . . . partisanship should be made explicit, since claims to nonpartisanship are hypocritical." To the Freudian, "The quest for knowledge cannot escape each individual's cumulative balance of pleasure and pain." To escape such traps, we must remember that:

> Any theoretical perspective leaves residues which are neglected for the time being. The pluralism of theoretical perspective provides a corrective for this neglect. And general assertions about man and society can be treated as provisional abstractions. (p. 12)

In the chapter on the "Changing Foundations of Scholarly Detachment," Bendix sees two major threats to scholarship. One is the change in man's "cognitive relationship to the world," bringing the entire enterprise of objective inquiry into question. The other is the crisis in "academic self government [which] threatens rational discourse as a basis of scholarship . . ." In the social sciences, continual experimentation in point of view, untested empirically or in scholarly discourse, leads to a radical subjectivism. The maintenance of standards, critical for the University's integrity, is endangered. In the University as a whole, "political expressionism," which confuses the limited arena of the campus with that of the national government, can easily destroy trust in the University by the community as a whole and, thus, also destroy the possibility of academic freedom. To abandon intellectual standards to participatory democrats, to use the campus as a staging-ground for national politics is to endanger the very possibility of objective inquiry.

In "Sociology and Ideology" Bendix notes that the power of the discipline lies not in its modest use for prediction and control but in the political consequences of social *theory*. As he states elsewhere, "Definitions of man and society are also social acts." In his own definition of sociology he emphasizes the autonomy of sociological propositions, logically distinct as statements of fact from either their origins or the means used to test them. We must, however, ask that they be in principle testable; the evasive language of the reductionists will not do. "Verbs like reflect, correspond, indicate, suggest, and many others are used to assert a relationship which is difficult or impossible to verify." And since we often cannot test our theories, responsibility requires "awareness of repercussions and a spirit of moderation in keeping with the fact that theories are trial presentations to the world" (p. 115).

In view of Bendix's general position it is clear that he could not be an exponent of deterministic evolution. In analyzing social change he employs the device of "paired concepts of interaction." Thus approaching Durkheim on the "normality of crime," he notes that crime is *both* a threat to the moral order *and* an occasion for rituals which strengthen that order. From this it is necessary to keep in mind "dual tendencies" in societies; they allow the development of

> a social scientist's critique of utopian thought. It asks us to consider *every* social phenomenon as transient. Social action evokes or provokes reactions: it has intended and unintended consequences . . . paired concepts and the perspective of dual tendencies are a theoretical adaptation to the Janus-faced quality of social life (p. 134).

As he notes, this view is not consistent with functionalism and the notion of systemic equilibrium.

Much of Bendix's work had dealt with social change, specifically industrialization as it occurred in England, Russia, Germany and the United States, as well as "modernization" in the new nations. He raises a key question: Can we generalize from the English experience to the remainder of the West—and from the West to that of emerging nations in Africa, Asia, and Latin America? He discards the "internal evolution" assumption, otherwise called the "stage theory," even for the West, even for England.

The internal evolution approach makes three basic assumptions: that societies are internally coherent sociocultural systems, whether traditional or modern; that change may be attributed to a society's internal functional differentiation, such as increasing division of labor; and that ultimately, social evolution (industrialization) will have the same effects wherever it occurs: ". . . once these prerequisites are given, industrialization becomes inevitable." These assumptions Bendix rejects. Instead, he sees societies as less than coherent systems:

> Societies can resemble the living people in Hofmannsthal's trenchant epigram: "The main difference between living people and fictional characters is that the writer takes great pains to give the characters coherence and inner unity, whereas living people may go to extremes of incoherence because their physical existence holds them together." Likewise, geographical contiguity, conventions and institutions hold societies together, even as they go to extremes of incoherence (p. 150).

Thus tradition and modernity are not mutually exclusive; they are dual tendencies. As for change as a product of internal differentiation, he notes the extreme interpretation of societies common today. "All complex societies have an internal structure and an external setting. Likewise all complex societies possess a formal structure of governmental authority which differs from and is relatively independent of, the group formations arising from the social and economic organization" (p. 291). He considers the inevitability of industrialization far from self-evident and the similarity of the outcomes of industrialization unlikely. "There is a sense in which it is true to say that because of timing sequence industrialization cannot occur in the same way twice."

In "Tradition and Modernity Reconsidered" Bendix gives us, indeed, a definitive critique of that amorphous *collage* of theory, fancy, fact, aspiration, and cynicism called "the study of societal development." Looking at the global situation, over time, he concludes that the very special English situation was grossly overgeneralized by Marx, who "tended to make necessities out of contingencies," and suggests a revision of vocabulary for the entire discussion. It runs like this: At the end of the 18th century, because of a long history of complex developments, a breakthrough occurred in England and also in France. "The economic transformation of England provided a 'model' for France, while the political revolution of France instantly became a major focus of political debate in England. Ever since, the world has been divided into advanced and follower societies" (p. 296). Accordingly, he continues, "modernization" indicates a valued change in some societies and imitative behavior in others. (He also notes that Holland was leader before England; Spain, perhaps, before Holland.) He notes, wryly, that one way to protect ourselves from the bias of overgeneralizing a little history is to understand better what really occurred, including the intellectual formulations of that occurrence, our "bias inherited."

As at the beginning, so at the end, Bendix has grounded his political analysis on a tough-minded sociology of knowledge. I am certain his book will be a permanent contribution to that sociology in part because he has worked with empirical problems of the greatest theoretical and substantive significance. His political theory is nowhere isolated and propositionalized; it is intrinsic to his entire analysis. This I find sensible and convincing, for he is a true macro-social scientist, as this volume superbly demonstrates.

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Theory Construction, from Verbal to Mathematical Formulations. By Hubert M. Blalock, Jr. (Englewood Cliffs, N.J.: Prentice-Hall, 1969. Pp. 180. $3.50.)

Many of the methodological procedures and points of view endemic to political science originally spread after contact with other disciplines. In the past sociology has provided frequent contagion. This book can make a further contribution, especially in our teaching at the undergraduate and beginning graduate-student levels and in some cases to ongoing research. It is also an interesting document in what it shows about its author, the sociologist with perhaps the greatest influence on the teaching of statistics among political scientists. Furthermore in reacting to the book many readers will come to appreciate the growing distinctiveness of research problems in political science and the signs of the maturation of the methodological branch of the discipline.

The book should be a useful addition to courses on research methods. It has two general concerns. The first is how one may state hypotheses so that they may be evaluated empirically. The second is how one may increase the level of abstraction of those statements. Thus Blalock's sensitivity to the distinction between operational and conceptual definitions underlies the work. The further transmission of such sensitivity could do much in the way of increasing the theoretical sophistication of empirical research in the discipline and perhaps lead to improvements in measurement as well.

"If the theory has been formulated in terms of underlying continua, rather than unordered categories and if measurement has of necessity been too crude to provide an adequate test, then a weaker test can always be made. But if the theory does not encourage greater attention to the improvement of measurement procedures then we cannot hope to advance as far as might otherwise be possible. In other words, I would prefer attempts at theory building that are in advance of present measurement practices, rather than those that take the status quo as a limiting factor."

The first three chapters discuss problems and strategies of translating purely verbal theories into more rigorous statements and of linking statements so that they form deductive systems. Some of the specific topics covered are the atheoretical implications of the regard for mere association and typologies. Alternative strategies are categorized as inventories of causes, inventories of effects, and simple chains and loops. The middle three chapters deal with issues more frequently found in the econometric literature; estimation, dynamic and equilibrium models, and feedback.

Specific topics here include identification, block-recursive models, lagged endogenous variables, difference equations, and differential equations. An especially exciting portion of the book is a rather long section of the next to last chapter in which the author gives examples of how one might make inferences about underlying models from the observation of stability. The concluding chapter analyzes three generalization strategies: that from the "element class" perspective (based upon the deductive model), the "class-subclass" perspective (based upon series of populations of decreasing membership), and the "indicator" perspective (based upon the conceptual-operational distinction). There are also two appendices. One treats statistical interaction and multiplicative models and has appeared before. The other is an introduction to calculus which should be sufficient for an understanding of the section on differential equations.

Several unusual characteristics of the author are plain in the volume. The clarity of the style and the use of numerous examples from the literature illustrate his concern for students and the care he takes as a teacher.

The illustrative examples also indicate his interest in the development of the social sciences generally. The chapter on the identification problem is heavily made up of cases from economic and political research. In fact, the book as a whole may be viewed as an introduction to econometric materials written from the perspective of less well-developed social science disciplines.

Yet it would be unreasonable to expect such a book fully to meet the requirements of our own discipline. Teachers will have to add other topics to fill out the coverage. My own view is that there are three main gaps. First, in reaction against our exclusively conceptual traditions we now clearly err on the operational side —even more than do sociologists. It seems to me that this presents us with even greater theory-building difficulty. Second, given our level of theoretical development, we need ways of underpinning our assumptions. For example the chapter on the identification problem does not discuss a readily available test of identifying restrictions which allows some evaluation of the necessary assumptions about exogenous variables. (Blalock does discuss the logic of the test; he discusses one of the political examples in a form in which it is "overidentified" when in fact the original was "just identified" and therefore precluded this test.)

Finally, from the subtitle some readers might anticipate that the book would deal with formal modeling, a theory-building strategy which has shown much potential in this discipline recently. As we should expect, however, it deals with theory construction from a purely statistical point of view.

Despite the obvious value of this work I must conclude that our problems are somewhat different from those in sociology. Because of our traditions, many of our problems, though not all of them, are more severe. The progress toward their solution may be enhanced by better informing ourselves of developments in the more advanced fields like econometrics. In this book we have an introduction that most political scientists will find useful, especially when supplemented by additional problems encountered in research on politics and public policy.

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Contemporary Political Theory. Edited by Anthony de Crespigny and Alan Wertheimer. (New York: Atherton Press, 1970. Pp. 320. $6.95.)

For many years the professional interest of analytic philosophers in political theory was indeed slight. In the last decade or two the situation has significantly changed. The change is of such magnitude that political theory may now be fashionable. Regardless, a marked tendency to apply to it the distinctive approach of analytic philosophy has emerged.

The anthology under review illustrates well the results of this application. Most of the essays consist of "second-order" activities rather than responses to substantive, "first-order" problems. Clarification of concepts and critical analysis of their logic or language receive preponderate emphasis. Prescription of political ends or the means of their achievement is unusual. Factual accounts of political institutions or behavior are rare. A great increase in methodological self-consciousness has been said to characterize recent philosophers and to induce a more limited conception of their powers. The implications of this self-consciousness for political philosophy are apparent in most of the essays in this volume.

After a general introduction by the editors, the book divides into seven sections. Each focuses upon a fundamental political concept—power, authority, liberty, equality, justice, rights, and political obligation. The sections consist of a three- or four-page introduction, two articles, and a select bibliography. The authors are English-speaking political scientists or philosophers. The articles have been previously published, and only one originally appeared before 1955. No less than five are reprinted here for at least the second time.

Capsule summaries cannot do justice to the contents of the essays in this volume. Their flavor may at least be suggested. The editors describe and evaluate, not uncritically, the major modern criticisms of traditional political philosophy. P. H. Partridge, in a subtle and challenging essay, denies "that measurements of political and social power . . . have, or can ever have, any very exact meaning" (p. 30). What may be needed, thus, is "the substitution of more manageable concepts for the portmanteau concept of power" (p. 37). Anthony de Crespigny analyzes seven forms of power. R. S. Peters maintains that the term authority "is always used to speak of ways in which conduct is regulated as distinct from the mere use of power—e.g., the giving of commands, the making of decisions and pronouncements, as distinct from the use of force, incentives, and propaganda" (p. 73). C. W. Cassinelli criticizes the failure to distinguish between the exercise and possession of authority.

K. J. Scott defends a nonmoral concept of the thing freedom which relies heavily on the everyday usage of the term. "Laymen are more expert at recognizing freedom than social theorists are at analyzing it" (p. 98). In an excellent article, Gerald C. MacCallum rejects the distinction between negative and positive freedom. Instead, he conceptualizes freedom as a triadic relation: it is "always *of* something (an agent or agents), *from* something, *to* do, not do, become or not become something" (p. 109). The essays on equality are particularly provocative. John Schaar advances a radical critique of the doctrine of equality of opportunity, which he sees as "a cruel debasement of a genuinely democratic understanding of equality" (p. 142). He later writes, "The heart of such a view of equality is its affirmation of equality of being and belonging" (p. 151). John Charvet goes further and rejects all substantive principles of equality as "incoherent and self-contradictory" (p. 167).

The thrust of D. D. Raphael's essay is the difference between conservative (penal, remedial, and commutative) and distributive justice, which aims at modification of the status quo. John Rawl's classic, "Justice as Fairness," is too well-known to attempt to summarize. On natural rights, Margaret MacDonald's analysis of the nature and justification of "value-utterances" is particularly stimulating. She regards them as "more like records of *decisions* than propositions" (p. 238). As such, they cannot be proved but can be more or less well defended. The main theme of Stuart Brown's tightly argued article is that "each man has an inalienable right to the protection of his moral interests, his person, and his estate" (p. 245). The anthology concludes with two first-rate and well-known articles on political obligation. They are Richard Wasserstrom's "The Obligation to Obey the Law" and Michael Walzer's "The Obligation to Disobey."

One weakness of this volume is the *nature* of the editors' otherwise commendable introductory essay. The problem is its failure to perform some functions of a good introduction. There is no explanation of the purpose and significance of the book or of the choice of articles and concepts. The account of the philosophical developments which conditioned most of the essays is inadequate. In this respect *Contemporary Political Theory* is less useful than, say, Anthony Quinton's *Political Philosophy*. The misleading character of the title is a less serious object of criticism. The essays in the anthology do not reflect the varied trends in contemporary political theory. Empirical theory and the Straussian version of political philosophy are not, among others, represented.

The choice of articles and topics in almost any anthology is subject to some question. In this volume, the work of H. L. A. Hart and Bryan Barry is conspicuous by its absence. Such fundamental political concepts as democracy, consent, participation, pluralism, and ideology are omitted. Still, the articles reprinted are generally of high quality and about concepts of obvious significance. They are also models of clarity. Then, too, the trend illustrated by most of the essays reflects the dominant philosophical movement of our time. The precise value of the concept of political philosophy which it implies is no doubt debatable. This collection of essays provides an additional, convenient source for further conduct of the debate. For these reasons the publication of this book constitutes a service to students of political theory.

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Between Philosophy and History: The Resurrection of Speculative Philosophy of History Within the Analytic Tradition. By Haskell Fain. (Princeton: Princeton University Press, 1970. Pp. 329. $10.00.)

Speculative philosophy of history is almost entirely identified with Hegel and the varieties of neo-Hegelianism, including Marxism. (Toynbee is not strictly speaking a speculative philosopher of history but a comparative historian whose conclusions are, at least in intent, empirical generalizations.) Analytical philosophy can be roughly divided into two wings. The first, sometimes called "ordinary language philosophy," derives from G. E. Moore and the later Wittgenstein. The second, which derives from the Vienna Circle and its diaspora in the war years, has been known as "'logical positivism" and "logical empiricism"; but it might just as well be called "ideal language philosophy," since in its view the function of philosophy is to clarify the logical form of scientific statements and theories, and this can be done only for an ideal scientific language which has been purified of the vagueness and ambiguity of ordinary language. It is not always recognized that analytical philosophy is no more monolithic than speculative philosophy, and that in fact a great deal of recent philosophical debate has been between the two wings. The positivist propensity, to give it its more traditional name, has been thoroughly domesticated in the training of behavioral scientists; it is taught, one might say, without criticism or alternatives, like lab technique to chemists. The critique of positivism—for which Peter Winch's *The Idea of a Social Science* serves as an introduction, as A. J. Ayer's *Language, Truth and Logic* has served for a generation for the positivist wing—is not well known and seldom understood by behaviorists. Meanwhile, historians still tend, although they are increasingly uneasy about the conceptual foundations of their discipline, to think that common sense and the techniques of archival research are all they need.

This preamble may help to explain Professor Fain's strategy in *Between Philosophy and History*. The positivist wing of analytical philosophy has never been comfortable with the concept of historical knowledge, because in its view all knowledge (explanation, prediction, etc.) conforms to a single *formal* model, and the essential features of that model, such as the presence of general laws and concept-neutral observation statements, just cannot be found in histories or persuasively prescribed to historiography as methodological goals. Hence, as Fain says (pp. vii, 206, 216), much recent philosophy of history has consisted of "leftovers from philosophy of science,"—a positivist philosophy of science which regards history as "sociology *cum* documents" (p. 5). In this situation, *Between Philosophy and History* is throughout, although implicitly, a rejection of the positivist propensity in philosophical analysis; the debate over the logic of explanation is ignored, except for a brief Epilogue added when nerve failed. Instead, the techniques of the other wing of analytical philosophy are used to restore the legitimacy of some of the insights of speculative philosophy of history. So Hegel is resurrected, looking leaner than his friends remember him, but also quite different from the inept caricatures on the "Wanted" posters which hang on the walls of graduate schools.

Before speculative history is reached, however, two-thirds of the book is devoted to the explanation and justification of the claim that philosophical analysis is *relevant* to history. The persuasiveness of this sort of argument rests on the conceptual analysis of instances and cases, which is carried through acutely and often wittily. In barest synopsis, the steps of Fain's argument are as follows: The minimal relation of philosophy to historical inquiry is that philosophy is the search for, and formulation and critique of, criteria for the intelligible application of concepts (p. 40). The concepts in question are not descriptive-interpretative concepts such as "Renaissance," "Enlightenment," "revolution," or "modernization," but meta-concepts such as "historical fact—historical hypothesis," "historical certainty—historical probability," "historical truth—historical fiction" (pp. 109, 111). The criteria for the intelligible application of such concepts are seldom explicitly stated but are "embedded" in decision procedures, the skilled employment of which is the discipline of historical research and argument (pp. 111, 138, 156 ff.). "Decision-procedures" are not modes of historical argument. Whether Martin Bormann is still living is an open question; no decision procedure exists to settle it. *That* Borman lived and was active before 1945 is not ordinarily open to question because of accepted decision-procedures in history for establishing the historicity of persons and actions.

So far so good. At this point, however, it becomes unclear what use Fain thinks should be made of the conclusion that the decision-procedures of historians contain within them concepts whose criteria of intelligible application are open to skeptical examination and possible change. At times it sounds as if the philosophical surgeon just pokes around in the historical corpus and sews it up again after satisfying his own curiosity (it is a "common mistake" to suppose that, because skeptical analysis brings the historical use of concepts to light, "the decision-procedures themselves need to be replaced," [pp. 141, 155]). At other times it sounds as if the patient is to be wheeled away with new liver and lights: the concepts of historical truth and historical fiction as used by historians, for example, cannot accommodate the assignment of quantified probability to historical hypotheses, and if a logic of historical probability were to be constructed, "the conceptual change brought about in the study of history would be revolutionary" (p. 171). It is clearly not within the intended scope of the book to settle this question; its aim is not to write a scenario for an historiographical revolution (any more than Kuhn's *Structure of Scientific Revolutions* tells physicists what to do next) but rather to fix with some exactness the *locus* and nature of whatever conceptual change *can* take place in a discipline not notorious for conceptual self-consciousness; and this has its own value.

In the second part of the book, beginning with Chapter XII, the argument returns to speculative philosophy of history, in a somewhat surprising way. Since the first part dealt with epistemological concepts applied to history ("truth," "probability," etc.), one might expect speculative philosophy of history to deal with problems of analyzing criteria for application of essentially historical first-order concepts such as "Baroque," "Puritanism," or "Frontier." Instead, the language of "criteria," "decision-procedure," and "application of concepts" virtually disappears. Speculative philosophy of history turns out to be ordinary historical narrative informed by self-consciousness about the "story-line" which unifies the narrative (p. 233). So if we accept the conclusions of the first part of the book, the major problem in giving an account of narrative understanding is to state the criteria by which it can be decided whether the account of a given incident belongs to (or is required by) a given narrative structure. What makes a story *one* story? How does the account of an episode as belonging to a particular story increase its intelligibility? Is narrative intelligibility a step toward theoretical explanation of an incident, or does it replace it? Although Fain succeeds in making the first question *arise*, and disposes of a number of possible criteria for the relation of an incident to a story (causal connection, integrated plot, a single continuing entity as subject), he does not in fact succeed in giving or even attempt to give even partial positive criteria. The answer to the second question similarly does not go beyond our unanalyzed intuitions of narrative coherence; and as for the third, he gives both answers by distinguishing between *story*, whose structure is wholly narrative, and *history*, which provides explanatory mechanisms of change (pp. 266 ff., 307 f.). This seems an unfortunate concession to a conceptual system which the rest of the book has struggled to call into question; it is itself a "leftover from philosophy of science." But although the questions of "speculative" philosophy of history survive the tentative answers which Fain essays, he does succeed in making plausible (as in his discussion of Marx's *18th Brumaire*) the thesis that it is story-lines rather than theories by which concepts are brought to the interpretation of "facts" in understanding the genesis and development of human institutions.

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Data Processing: Applications to Political Research. By Kenneth Janda. (Second ed., rev.; Evanston: Northwestern University Press, 1969. Pp. 294. $8.50, cloth; $3.45, paper.)

Karl Deutsch used to argue, perhaps in jest, that we ought to call a moratorium on improvements in research technology while we got some political research done. The speed with which the computer world makes obsolete its products including personnel training is almost overwhelming. A political scientist whose real interest is in the decision process or voting studies or causes of civil disorder may find himself spending a great amount of time becoming acquainted not only with the phenomena and methods with which to study them but also with computer technology which is a necessary tool for some of the methodology.

Manuals and introductions, therefore, are essential, but by their very nature and the nature of the world they describe they are almost always obsolete very shortly after their creation. Perhaps introductions to data processing for social scientists should have a loose-leaf format, but this would not solve the problem. Change occurs not only in the technical means of processing but also in the most basic concepts of that processing. Once it was necessary to know how to wire a board; learning that kind of thing now is surely a waste of time.

Kenneth Janda recognizes at least a part of this problem in his preface. Revisions for this second edition, he says, were prompted by advances in machine technology. This revised edition pays attention to third generation computers, tapes, and discs, but the basic punchcard outlook remains the essence of the introduction to data processing. The reader is taken through a series of steps that were once important to data processing. Moreover, the descriptions are very well written and should be clear to the most unsophisticated beginner. The person unfamiliar with the meaning of those little holes in the household bills would clearly profit from reading Janda's second chapter entitled "Recording Political Data on Punchcards." If he is a nonquantitative political scientist, he will get a clear glimpse into a whole new world of research technology.

Janda's next two chapters are interesting. Even after consorting with computers for five or six years, I still found in this book new details about how they work. It is not clear that knowing how a counter-sorter uses brushes to send a card to the right bin will increase my research potential, but it surely reduces the romantic magic of the thing. Again in these chapters Janda describes some decently complicated hardware and Fortran programming and does so in such a way that, although the reader becomes neither programmer nor mechanic, he does have some appreciation of what happens in the blue and grey box.

These two chapters are not of equal utility, however. The second, which deals with electronic computers, is much more likely to speak to the data processing environment of most universities than is the first, which describes unit record equipment. With the exception of the keypunch and verifier and possibly the counter-sorter, the machines discussed in Chapter III simply will not be available to most readers. Computers can do the same jobs much more accurately, quickly, and cheaply. Most computer centers will therefore probably continue the trend to abandon sorters, reproducers, tabulators, interpreters, and collators and put peripheral jobs onto the main computer or auxiliary computers. Furthermore, the proportion of these jobs to total work will greatly decrease as data processing relies more and more on discs and data cells and on interactive time sharing machines. It is unfortunate, therefore, that the discussion of unit record equipment could not have been replaced by or at least supplemented with a discussion of computer terminals.

The remaining four chapters and most of the appendices are full of useful hints on coding, collecting, managing, and processing punchcard data. Although "keypunches" for entering data onto tape, badge readers, optical scanners, and laboratory input devices are now available in many universities, large inputs will probably require punchcards in most installations for the foreseeable future (that is, a year or so). Hence, the neophyte in data collection will find these chapters, and especially the one entitled "Suggestions for Collecting, Coding, and Managing Punchcard Data," very helpful. Many new program packages for data analysis have been devised, however, and one might do well to consult Norman H. Nie, Dale H. Bent and C. Hadlai Hull, *Statistical Package for the Social Sciences* (New York: McGraw-Hill Book Company, 1970) or to look at the new third-generation version of Datatext developed by David Armor and others in the Social Relations Department at Harvard. Each of these systems is English-language oriented and includes a variety of statistical procedures. Of course, the use of one of the better statistics texts is essential if one is not already acquainted with that discipline.

Janda makes the argument that understanding punchcards is the best approach for understanding computing. Data on magnetic tape, he says, cannot be "seen." Students learn most easily by looking at a punchcard and watching it —with its fellows—in a counter-sorter. I have made the same argument many times before, but then Janda's and my graduate educations are the same vintage. Is the punchcard the best prop for learning when the student has access to a terminal? As the punchcard and the counter-sorter increasingly become at best an analogy of what is really happening, the old means of teaching will be misleading. The "counter-sorter psychology," as Janda puts it, does have limitations, and it has been responsible for the under-use of the potential power of the computer.

Nevertheless, this book should be judged on what it was meant to do when it was published. It was written for people with no prior knowledge of data processing—not to teach them sophisticated analysis, but to show them "the early stages of data processing, particularly the recording of relevant data on punchcards," and that it does very well indeed. It is a good, if dated, basic introduction to these things. But then as Janda himself has written, ". . . this book will surely be rendered obsolete—and the sooner the better."

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Explanations. By Gwynn Nettler. (New York: McGraw-Hill Book Company, 1970. Pp. 237. $7.50.)

The sense of "explanation" used in this book is very wide, so wide in fact that it amounts only to satisfying the questioner's curiosity by telling him a story, whether the story be true, false, or incoherent. In the author's view, there are not only good and bad explanations, but good and bad types of explanation; and most of Nettler's account is devoted to discussing two reputable types—definitional and scientific—and two disreputable types—empathetic and ideological. His final chapter is concerned with the dangers of letting social scientists guide our social policy, dangers which arise, he says, from their habitual use of empathetic and ideological explanations in support of their moral or political goals.

It is difficult to see the utility of Nettler's fourfold classification. The four types are obtained by employing several different principles of classification, but we are given no reason to think that to each type there corresponds a distinct principle. Ideological explanations, for instance, are characterized as "group-supported patterning of beliefs of inadequate empirical warrant, where such beliefs are energizing, in attack or defense of values, and comprehensive" (p. 177). It is obvious that such a characterization does not exclude the other three types; for persuasive definitions, inadequately supported scientific explanations, and empathetic "understanding" can be elements within an ideological explanation. The latter is simply a use to which the other three types are put. Moreover, why must an ideology have "inadequate empirical warrant"? It is the end to which the ideological explanation is put that helps to mark it out, not the truth or falsity of its content. Marxism would not be less of an ideology if all its assertions were true.

Again, the "distinctive feature" of scientific knowledge is said by Nettler to be "generalization from facts according to rules." Not only does this hold equally well of empathetic and ideological explanation, it suggests (a) that singular observation statements are not part of scientific knowledge, and (b) that science grows by generalizing from such nonscientific statements. The two claims are incompatible, and in any case both are false. Nettler's difficulties are enlarged by his apparent belief that "many explanations—perhaps all—are fictions; they describe events, put them together, *as if* the descriptions were complete, uniquely accurate, and the relationships the only patterns discernible" (p. 6). Yet in what sense could this possibly be true? The notion of a complete description is extremely dubious, and the fact that an explanation deals with a limited portion of what we can describe hardly makes it fictional—unless that term merely means "selective."

When the author comes to discuss each type of explanation in detail, the initial confusion is compounded. For example, in the chapter on explanation by definition he says that showing a sample and uttering its common name "has the advantage of saving one from the definition of fictions. It is difficult to point to a ghost" (p. 10). It is not difficult, however, to point to objects of mass hallucination like the Virgin Mary seen in the sky, and it is difficult to point to electrons, which are not fictions. The supposed advantage is itself fictional.

In the same chapter a purely logical feature of operational definitions is taken to be a matter for empirical testing. For Nettler thinks that the truth of the statement "The use of operational definitions reduces predictive power" can be decided "for the pragmatist not by debating the logic of prediction, but by testing the relative contribution of broad and narrow, fuzzy and precise, concepts to the solution of a forecast problem" (p. 23). But the reduction in predictive power is a simple logical consequence of making the sense of the *definiendum* synonymous with a given set of test-statements. It results in our being unable to use the *definiendum* to refer to anything not described in those statements, and hence in our being unable to use it to predict anything which falls outside them. Empirical testing is irrelevant to this reduction in predictive power. Nettler makes a closely related error in replying to Karl Popper's argument (see *The Logic of Scientific Discovery*, London: 1959, p. 440) that all operational definitions are circular. His point is that the results of the operational test cannot define the *definiendum* since they must themselves be tested by the original procedure. For Popper this circularity shows us the hopelessness of trying to define universal terms by means of any possible experience. Nettler replies that scientists, in fact, do not go on making endless tests; at some point they are satisfied. Of course they are, but their practical satisfaction does not eliminate the logical circularity of its consequences for the doctrine of operationalism.

The chapter on empathetic explanation is rightly critical of its use. But the criticism does not bring out the basic difficulty in taking empathy to be a type of explanation. This is simply that while understanding other people's motives, beliefs, and feelings by analogy with our own may lead us to understand their behavior, this is a method, however poor, of arriving at a recognized type of explanation—one in terms of reasons or causes. It is not itself another type, since the information it provides us with is no different from that given by scientific explanations, for example. More generally, Nettler does not distinguish, under each of his four headings, among the kind of information obtained, the method used to obtain it, and the use to which it is put. Scientific explanation, like empathy, is distinguished by its method; definitional and ideological explanation are distinguished by the uses to which certain kinds of information are put. Neglect of these differences produces a deep-seated muddle from which the author's occasionally shrewd remarks struggle to free themselves.

These problems aside, it is curious that a book by a sociologist should be composed, on the one hand, of such a large number of misguided general comments on the philosophy of science, and on the other hand, of an even larger number of quotations concerning specific social questions on which little or no comment is made. The construction of this book remains as obscure as the audience for which it might be useful.

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Essays in Theory and History: An Approach to the Social Sciences. Edited by Melvin Richter. (Cambridge, Massachusetts: Harvard University Press, 1970. Pp. 291. $10.00.)

These essays in theory and history are an outgrowth of a Harvard interdisciplinary course taught by some of the contributors under the leadership of Samuel Beer who first offered the course in 1946. According to Richter's introduction, the central purpose of the course is the development and application of comparative social and political theory to an analysis of historical events. But the main thrust of the essays—contributed by political scientists, historians, and a sociologist—is the use of history to test and develop contemporary theory rather than the application of theory to history.

In the opening essay Samuel Beer persuasively argues that the antihistorical and parochial approach of American political science "cries out for a deepening of historical perspective and a new stress on comparative political history." Walter Dean Burnham drives the point home in his study of the presidential election of 1964. In the course of his historical analysis—which reveals that the "unexpected" characteristics of the 1964 election are quite similar to geographic alignment patterns of the 1896–1932 era—he throws serious doubt on the validity of an ahistorical survey research approach that implicitly assumes a high degree of stability in the mass base of political coalitions during recent American elections.

In a fascinating study of the social mobility of the working class in America, historian Stephan Thernstom takes a position similar to Burnham's by stressing that generalizations about the relationship between social mobility and class solidarity today are unreliable without a thorough understanding about class and mobility in the past. Following a suggestive lead made by Clark Kerr, Thernstom hypothesizes that the development of working-class solidarity not only requires a relative permanence of membership in the class, but also demands class stability in one setting—a geographic permanence that fosters a sense of class identity and common hostility to the "ruling class."

In his studies of mid-nineteenth century Newburypost and Boston, he found that the urban population was highly volatile, especially among semiskilled and unskilled members of the working class. For example, in Boston—which he assumes to be a typical city—he found that during the period 1837–1921 more than a quarter of the working-class population at any one date had not been living in the city a year before. As for occupational mobility, his findings indicate that more than 40 per cent of working-class sons who did stay in Boston were able to obtain and hold middle-class jobs. Most of those who were unsuccessful in the struggle to better themselves were not inclined to remain in the city. Because they did not stay in any one locale, they were never able to make their weight felt; in Thernstom's words, they were "tossed helplessly about from city to city, alienated but invisible and impotent." Thus he argues that the combined effect of high rates of geographical and occupational mobility have been decisive in retarding the development of a sense of solidarity among working-class Americans.

"The Americans," pondered Tocqueville in one of the latter chapters of *Democracy in*

America, "are not virtuous people, and nevertheless they are free." It is on this apparent paradox that Melvin Richter focuses in his perceptive piece on Tocqueville's use of Montesquieu's theory. The paradox must have given Tocqueville considerable difficulty. For he came to America, we are told, with the assumption—a basic premise of most classical writers, and especially of Rousseau—that a healthy democratic polity was dependent upon the presence of citizens motivated by the highest civic virtue and dedicated to the attainment of the common good. And yet he was confronted everywhere in America by self-seeking individuals, grasping for economic gain and status.

Tocqueville resolved this dilemma by applying to the American scene Montesquieu's insight that private concern can lead to public good. Thus, he concluded that political participation, initially activated by private interest, could educate citizens to accept a broader view of the nature of the polity and to realize that their own private interests are better served by being subordinated, at least in the short run, to the public interest. Unfortunately, Richter refrains from a critical analysis of Tocqueville's position, although he does not resist a passing swipe at recent critics of elite theories of democracy.

The essay by Michael Walzer describes a form of participation—revolutionary repression—that is in sharp contrast to Tocqueville's interest model. Revolutionary repression, which is equivalent to "terrorism from below," 'is a system of mutual control and self-restraint in which each controls the other and no one escapes. Its existence depends upon voluntary and active participation by citizens and upon a widespread sharing of power and authority. When Everyman becomes a government agent, "spying is replaced by mutual surveillance—by what the Puritans called 'holy watching.' The threat that underlies revolutionary repression is not death but ostracism: excommunication from the congregation, exclusion from the party, exile from society" (p. 127).

In an otherwise brilliant study, Walzer brings his essay to an abrupt close with the dictum that "revolutionary discipline is the price of an activist and equalitarian self-government" (p. 136). To reject this option, he argues, is to accept an equally unpleasant choice: a Lockean system in which elites rule and citizens become privatized and apolitical. I have difficulty taking Walzer seriously. If there are no other choices than the two he presents, democratic theorists ought to close up shop.

The other essays in the volume by Klaus Epstein, Charles Tilly, and Sydney V. James are competent but less interesting. As a whole the collection is not outstanding, but, as I have indicated above, some of the pieces are worthwhile and challenging. The importance of history as it related to social science—which the essays skillfully and forcefully demonstrate—cannot be ignored by either the behaviorally oriented political scientist or the political theorist.

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Politics, Personality, and Social Science in the Twentieth Century: Essays in Honor of Harold D. Lasswell. Edited by Arnold A. Rogow. (Chicago: The University of Chicago Press, 1969. Pp. 445. $11.00.)

This aptly titled *Festschrift* promises us an analysis of Lasswell's work by a distinguished and diverse group of essayists including former students, collaborators, and others who have been influenced by the Lasswellian contributions to the social sciences. Given the diversity of contributors, one might well have expected this volume to have been even less unified than most *Festschrift* collections. That this is not the case is a tribute to the editor's skill in having identified the most significant aspects of Lasswell's work and in having commissioned essays designed to illuminate the most significant dimensions of his writings and influence.

The largest category of essays in this volume consists of explications of those aspects of Lasswell's work that have been especially influential in stimulating and shaping each essayist's own work. In this category are Edward Shils' "Reflections on Deference," Myres McDougal's continued reflections on "Legal Education for a Free Society," Karl Deutsch's analysis of "Some Quantitative Constraints on Value Allocation in Society and Politics," Rogow's "Toward a Psychiatry of Politics," as well as substantial essays by Roy Grinker, Robert Rubenstein, Daniel Lerner, Allan Holmberg, and William Fox.

Another category of essayists is distinguished by the critical evaluation of unique dimensions of Lasswell's contributions to the social sciences. Morris Janowitz provides us with an incisive and probing evaluation of Lasswell's pioneering work on "Content Analysis and the Study of the 'Symbolic Environment.'" Janowitz emphasizes the "powerful impact" made by Lasswell's "intellectual aspirations for the study of communication" (p. 163), but concludes that "quantitative content analysis failed to achieve its expected potentialities . . . [or] to emerge as a device for describing the essence of mass communications on a long-term trend and social change basis" (p. 156). Janowitz' conclusions are challenged by another essayist, Ithiel de Sola Pool, in an impassioned and loosely reasoned analysis of "Content Analysis and the Intelligence Function." Even more lively is the sharp disagreement between Robert Rubenstein and Arthur Brodbeck on the efficaciousness of Lasswell's well-known prescriptions for the sharing of power as a means of fostering the development of generally democratic institutions and personalities, a dispute to which we shall shortly return. Editor Rogow's inclusion of essays that present conflicting interpretations and evaluations of Lasswell's work contributes significantly to the usefulness of this collection.

Especially useful for the general reader are the essays that are addressed to the most important dimension of Lasswell's life work, *viz.*, his overall purposes, his underlying objectives, in short, his intention. The most ambitious, if not perhaps altogether original, of these contributions is that of Bruce Lannes Smith, who deals at considerable length with "The Mystifying Intellectual History of Harold D. Lasswell." In the same category is Arthur Brodbeck's curious but challenging consideration of "Scientific Heroism from a Standpoint within Social Psychology." These broadly interpretive essays share Janowitz' view that "there is a powerful continuity in focus in Lasswell's theoretical writings. His style of theorizing emphasizes the substantive goals of social and political institutions" (p. 167). These essays present a necessary corrective to the shallow, though remarkably common, opinion that Lasswell's work is significantly characterized by a "versatile and irrepressible eclecticism." (See this *Review*, March, 1963, p. 127 for a typical expression of this view by Professors Schaar and Wolin.) Heinz Eulau's essay in this volume underscores the inadequacy of this vulgar opinion by emphasizing the general "failure to understand and appreciate the philosophical underpinnings of [Lasswell's] theoretical, methodological, and substantive writings" (p. 17).

The dimensions of a *Festschrift* volume do not, to be sure, afford any of the essayists adequate opportunities to analyze these "philosophical underpinnings" in sufficient depth. Still, a major subsection of Bruce Smith's essay deals provocatively with what Smith has identified as "The Meeting of Freud and Marx" in Lasswell's work.[1] Smith, among others, emphasizes that—from the beginning of his career—Lasswell has been unceasingly concerned with the development of a social science, the normative components of which are designed to encourage and to assist mankind in shaping a social order within which "the dignity of man" can be realized. Such a social order is variously termed by Lasswell as "the Free Man's Commonwealth" (p. 73), "a commonwealth of mutual deference" (p. 138), or even "a world commonwealth of human dignity" (p. 195).

Lasswell has termed his formulation of "The Free Man's Commonwealth" a "developmental construct." He himself regards this empirical-normative formulation as his most creative and original contribution to the social sciences, and a number of the essayists rightly devote considerable attention to it. Daniel Lerner describes the developmental construct as "the core of policy-science thinking as conceived by Lasswell" (page 171), (see also pp. 68, 132–33, 172, 205). There is a distinctly Deweyan element in the substance of Lasswell's preferred developmental construct, for the social order that will best realize what he terms "the dignity of man" is one characterized by the most perfect "shaping and sharing of values" (see esp. pp. 51–52, 164). Lasswell's "Free Man's Commonwealth" is located at one extreme of a continuum, and at the other extreme is the "Garrison State." The polar opposite of the "Free Man's Commonwealth," the "Garrison State," is characterized by extreme inequality, and by a generally totalitarian, authoritarian rule, one dominated by "specialists in violence" (see esp. pp. 173, 205–7). Lasswell's lifelong concern with the emergence of such regimes has found its strongest expression in his political psychology in the form of "the politics of prevention."[2]

Perhaps the most critical dimension of such "preventive politics" for Lasswell is the development of what he understands as "democratic character." It is pertinent in this context briefly to touch on the already noted disagreement between two of the essayists on the practical and political applicability of this dimension of Lasswell's formulation. Dr. Robert Rubenstein, a psychoanalyst who collaborated with Lasswell in an attempt to rectify conditions they considered undemocratic in a psychiatric hospital[3] reports that the hospital's "basically authoritarian character remained substantially unchanged despite the modifications introduced" (p. 151). Rubenstein takes note of the reluctance of the senior doctors to give up "power which might jeopardize their ability to meet demands made by those in the larger community to which they were responsible" (p. 152). "The critical factor, however, was the ambivalence of patients about further power sharing . . . past experience as losers, victims, and sufferers contributed to low expectations regarding their capabilities and to relatively modest demands" (pp. 152–53). Just what might have been expected, replies another of the essayists, Arthur J. Brodbeck, who finds that Rubenstein's conclusions point to a fundamental and continuing problem in the Lasswellian understanding of "democratic character" and "value sharing.'" Brodbeck reports that

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[1] A comprehensive analysis of the Lasswellian synthesis of Freud and Marx may be found in Robert Horwitz, "Scientific Propaganda, Harold D. Lasswell," in *Essays on the Scientific Study of Politics*, ed. Herbert J. Storing (New York: Holt, Rinehart and Winston, Inc., 1962), pp. 242-63.

[2] Harold D. Lasswell, *Psychopathology and Politics* (Chicago: The University of Chicago Press, 1930), see esp. chapter X.

[3] See Robert Rubenstein and Harold D. Lasswell,

> "For a long time, I believed along with Lasswell, that the more power one shares, the more power one has. Such a philosophy promotes the impulse to compromise with too much eagerness. If one shares power with those whose character is less developed than one's own, whether they be individuals or groups, one's own character undergoes a change and it is *less* frequently for the *better*, even given the strongest wish to maintain a liberal democratic outlook" (p. 253), (emphasis in the original).

Basic disagreements such as this on the part of serious interpreters of Lasswell point to the necessity of deeper and more comprehensive evaluations of the foundations of his work than can be explored within the confines of a *Festschrift* volume. Indeed, a critically important element of his work that is dealt with least adequately in these essays is the subject of propaganda, a phenomenon that has fascinated Lasswell since the very beginning of his career nearly half a century ago. Janowitz correctly notes its *centrality*, remarking that in Lasswell's work propaganda "is not seen as some foreign matter, some intrusion into the body politic, but rather as a generic aspect of the political system . . ." (pp. 166–67; see also p. 377, "In Quest of a World Myth"). It is, in fact from this point that one may trace virtually all the major threads that have been woven into the remarkably rich and complex tapestry of Lasswellian social science. For, in Lasswell's view, the true meaning of propaganda rightly understood does not consist in the attempt to " 'put something over,' but to find out what will stay put in social practice."[4] It is through an adequate analysis of Lasswell's understanding of propaganda that one may at last fully discern and even evaluate his remarkable attempt to find solutions ot the pressing problems of "Politics, Personality, and Social Science in the Twentieth Century."

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On the Medieval Origins of the Modern State.

By Joseph R. Strayer. (Princeton, New Jersey: Princeton University Press, 1970. Pp. 114. $5.00.)

It was Hegel who first posed the question dealt with in this book, when he distinguished "the modern state" as both *sui generis* and a specifically European product. He, together with many 19th-century historians, saw something distinctive above all in the attitudes involved in modern state procedures, in the kind of authority bestowed and the kind of loyalty required. The topic was magisterially discussed by Weber, who, in opposition to the Hegelian legacy, defined the modern state in real-political terms, but also laid special emphasis on the mentality of the participants—on their norms of "rationality" and "legality." He too dealt with the question of the specifically European origins of the phenomenon; with his comparative frame-work and conceptual lucidity, he analyzed the political developments of the medieval–early modern periods in terms of a merging of "patrimonial-traditional" with "rational-legal" techniques of authority, the latter gradually dominating and excluding the former. Weber also replaced Hegel's notion of a unique and necessary progress with that of a reversible and repeatable succession.

Professor Strayer begins by discussing, with verve and insight, the characteristics of modern states. He proceeds to show how modern state institutions emerged during the early and central Middle Ages out of earlier loyalties and groupings, and here he rightly emphasizes the importance of such features as the notion of royal justice and the shift of religious authority from the king after Christianization; medieval governments were "law-states." His use of some of the framework of the modern state as an analytic tool throws light on medieval governmental processes; it throws light in particular on the trends toward centralization and toward a ruler-ruled relationship based not on force but on a kind of consent, which could

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The Sharing of Power in a Psychiatric Hospital (New Haven: Yale University Press, 1966).

[4] Harold D. Lasswell, "Propaganda," *Encyclopedia of the Social Sciences* (New York: The Macmillan Company, 1934), vol. XII, p. 527.

border on ideology or nationalism. This approach seems useful partly because it reflects to such a degree the value-choices of medieval governing and politically conscious groups.

Sections II and III, however, of this inordinately brief book fall away sadly from the perspective outlined in earlier pages, becoming more and more a mere collection of asides, arrived at, apparently, by subjecting a somewhat arbitrary selection of English and French constitutional *causes célèbres* to the question (in this isolated form rather banal), "How did it contribute to the rise of the modern state?" The author, consequently, fails to maintain any semblance of a general inquiry. England and France are taken to stand for Europe, on the grounds that other states followed their examples; this half-truth both does scant justice to developments elsewhere, and leads the author into serious error. For instance, when he says "One would scarcely expect a medieval government to concern itself with problems of health or education" (p. 77), and again, "all these attempts to regulate foreign trade produced very little in the form of administrative structures" (p. 79), this implies a total disregard of the German and Italian city-states—about which one author has recently stated, "one presupposition was constant intervention by the commune in every aspect of social and economic activity."

Second, and more seriously, the attempt to penetrate the ideology of government and governed in any profound sense is abandoned. Section III, dealing with the new monarchies from 1450 onward, degenerates into a series of increasingly trivial observations upon English affairs. On the question of rule by councils (committees), certainly Spain and (since the author is aware that the phenomena under discussion are those of the Ancien Régime in general) Prussia deserves mention. The concept of nationality, surely a central feature of the modern state, gets only a cursory mention (p. 109), and this implying that it was first of significance in the 19th-century—what of Bohemia and of Machiavelli? A book may be aimed at students, but no reading public deserves this kind of treatment.

This book, therefore, after a promising first section, really contributes little to the question of the origins of the modern state in medieval Europe. This is a greater pity since a new study of this phenomenon, in the light of modern scholarship, could help to answer some important questions. For example, what really is distinctive about this "modern state"? The administrative techniques, discussed in part in this book, can surely be paralleled in the ancient city-states and empires. As for governmental mentality, a good deal more than modernity is required to put a state into a distinct category. To what extent, furthermore, is the "modern state" a specifically European phenomenon? To what extent does it rely on a specific ethical and cultural tradition?

If anyone wants to see a competent statement on this subject, they should consult J. A. Maravall, "The origins of the modern state," in *Cahiers d'histoire mondiale,* 6 (1961), 789–808.

ANTONY BLACK

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The Democratic Citizen: Social Science and Democratic Theory in the Twentieth Century. By Dennis F. Thompson. (Cambridge: Cambridge University Press, 1970. Pp. xi, 271. $8.95.)

This short book is a systematic analysis of the bearing of recent empirical research upon twentieth century British and American theories of democratic citizenship. It is thus an attempt in a specific area to bridge the artificial gap that still unfortunately persists between some political scientists whose work concentrates on quantitative research and those who follow traditional methods of inquiry. Thompson assumes that no unbridgable gulf need exist between traditional democratic theory and the relevance of evidence from behavioral social science. He rejects both "prescriptivism," which assumes that the norms of democratic citizenship "are immune to challenge by evidence from social science," and "descriptivism," which assumes that values advanced by democratic theorists are, in effect, descriptive propositions that can be refuted by descriptive evidence. Recognizing that the basic presuppositions of democratic "citizenship theorists" can not ultimately be refuted by the findings of social science, he examines the ways in which social science can support or weaken the standards asserted.

The democratic theory that Thompson explores centers on "the active involvement in political life" of citizens and their "present and future capacity for influencing politics." (He deliberately excludes the other side of democratic theory, the problem of the degree of governmental responsiveness to the desires and needs of citizens.) The theory of democratic citizenship is, of course, not that of the democratic straw man—always thoroughly rational, fully informed, far-sighted, and devoted to a common good—that is so often set up as the traditional democratic model and so readily dismissed by contemporary critics. Nor is it that of recent democratic elitists whose position Thompson rejects. He draws his generalizations about democratic "citizen theory" from a group of specific writers who range from Dewey to Kelsen, from A. D. Lindsay to G. D. H. Cole and Harold Laski, from Graham Wallas to Carl J. Friedrich. Emphases among these naturally differ; but they appear to share a commitment to the basic presuppositions of individual autonomy and improvability. Thompson's empirical evidence comes from an impressively wide range of reports on research in psychology, sociology, and economics, as well as political behavior.

Thompson examines in detail the bearing of empirical evidence upon the demands for a high level of participation by citizens, for confidence in their capacity for rational discussion and rational voting, and for equality. His treatment of these demands is sophisticated, frequently original, scrupulously fair in the presentation of opposing arguments, and, therefore, complex. The complexity is brought under control by his schema that requires discussion of the four elements of citizenship noted above in terms of three modes of citizenship: (1) existing conditions; (2) current trends and "constructive ideals," that is, changes that could be made to improve the extent or the quality of citizenship within the present social structure; and (3) "reconstructive ideals," that is, changes that would require radical reconstruction of society as proposed, for example, by Laski and Cole. "Without reconstructive ideals, a democratic theory is devoid of vision. . . . With *only* reconstructive ideals, a democratic theory spells disillusionment" (p. 51). Empirical evidence naturally has most to say about the first mode and is, at present, least pertinent to the third.

Several examples will illustrate Thompson's analysis. He acknowledges that the volume of participation in the United States and Britain is not impressive; but concludes that, if one considers the democratic *functions* of participation, then the minimum criteria are satisfied. If a majority of voters disapproves the actions of those in power, this is at least a negative indication that established rules are not being violated. Failure to register disapproval may, of course, result from the exclusion of some interests. On the other hand, participation of some who appear to be excluded can occur through membership in an interest group or through symbolic representation in a group with which nonmembers identify. The evidence shows that other functions of participation, for example, to promote stability and self-realization, are, in general, strengthened by participation.

Following his general schema, Thompson considers current trends and proposed changes that might increase participation, and concludes with a brief comment on "reconstructive" proposals that involve primarily the dispersal of political power. He considers separately the functions of the process of discussion —for some theorists "*the* essential of democracy"—of "rational propaganda," and of rational voting. He suggests that although most voters do not calculate the relation of their votes to their self-interest or to general interests, if the reasons that can be attributed to their actions are compatible with the empirical evidence, that compatibility in itself constitutes a rational explanation of voting behavior. He concludes that "the process of discussion contributes toward rational voting by discouraging manipulation and by encouraging formation of opinions and preferences by reference to the opinions of one's groups" (p. 143). He also holds that voting is rational in the sense that voters can be said to express their interests when they vote on the basis of status and partisan attitudes. Finally, he maintains that "voters" express the general interest in a procedural sense by accepting the electoral verdict" (p. 143). He distinguishes between *de jure* and *de facto* equality of opportunity to participate in democratic citizenship. For the existence of formal *de jure* opportunity he finds support in the empirical evidence. Support for the latter he finds, on psychological, economic, and social grounds, to be seriously deficient, especially in respect to persons of lower status and black persons. His analysis of the separate elements of theories of democratic citizenship, of the pattern of their interrelationship, and of the bearing of the evidence upon them is actually much more intricate and challenging than these condensed illustrations suggest.

For each of the elements of democratic citizenship, his analysis relies heavily upon evidence of the impact of trends in such areas as urbanization, economic mobility, extension of governmental activities, and the consequent increased salience of government, the media of news and opinion, educational opportunities, and group activities. His "constructive ideals" for extending citizenship and for improving its quality are realistically related to such trends. His conclusions about the present status and the future of democratic citizenship is, on balance, cautiously reassuring. Despite discrepancies between democratic ideals and some of the empirical evidence, and despite the fact that some of the evidence gives only weak empirical support, Thompson concludes that "most of the essentials of citizenship theory remain intact."

Tightly condensed, rigorously argued, meticulously documented, this significant book should correct many hasty conclusions and loosely argued claims.

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Managing the Modern City. Edited by James M. Banovetz. (Washington: International City Management Association, 1971. Pp. 467. $15.00.)

Since 1934 the International City Managers Association has been developing basic materials for the training of municipal administrators. Over the years these materials have grown into the much respected Municipal Management Series, a 15-volume shelfload of how-to-do-it manuals. They have been widely used for pre-service and in-service training, especially the venerable leadoff volume, *The Technique of Municipal Administration,* which through four editions—the first in 1940 and the latest in 1958—has provided several generations of city managers, personnel directors, city planners, budget officers, and administration analysts an introduction to the art and science of administration.

In the light of this history, the publication of *Managing the Modern City* is of considerable interest in the field of public administration. As a replacement for *The Technique of Municipal Administration* it is more than a revision of the traditional how-to-do-it approach to city management; it is a greatly broadened interdisciplinary overview not only of the role and function of the manager but also of the setting in which he functions. It raises the role of the urban manager from that of a detached and routine functionary to that of a broad-gauged, many-faceted, and heavily involved generalist.

Because it seeks to deal with urban management in so broad and deep a context the volume becomes several books in one. The first "book" develops the historical and social framework within which urban management proceeds today. The opening chapters, written by the volume's editor, James M. Banovetz, tersely recap the forces and trends that have produced today's complicated and turbulent urban setting, and other chapters make frequent reference to the socioeconomic and political constraints on modern management. The authors repeatedly emphasize, from a dozen different points of departure, that management begins when the manager understands the forces at work that impinge upon his function. Where the earlier *Technique* sought to reduce administration to a set of operating procedures and theorems, this new volume sees modern management as open-ended and infinitely varied, involving human perceptions and subjective value preferences as well as matters such as procurement procedures or computer runs that can be objectively measured. Management in these pages is fully restored as both art and science.

A second "book" represents an even more fundamental departure from the approach of the predecessor volume. This is an extensive analytical overview of administrative organization and leadership, in which several of the contributing authors summarize the central findings of social science research in recent decades. Especially noteworthy in this connection is Kenneth K. Henning's chapter dealing with organizational dynamics and types of leadership, although its value would have been even greater had the author related his theoretical points to the various forms of organization in operation today in American cities.

How-to-do-it guides constitute another "book," but this one the reader must search out, because even in the sections dealing directly with operational matters—personnel, public relations, budgeting—the emphasis is less on the practicalities of everyday administration and more on the larger forces that affect policy-making. In the chapter on financial administration, for example, James E. Jernberg deals with budgeting as a tool of management and analyzes the factors that constrain budget decisions. His knowledgable introduction to PPBS is typical of the volume's effort to cover relevant developments and ideas that reflect the growing complexity of the management function. Where the predecessor volume dealt largely with such practical details as accounting ledgers and the encumbrance procedure, this volume identifies underlying dilemmas and operating dynamics.

The final "book" is a series of clearly drawn introductions to some special areas of central concern in government and administration. Several of them should be especially useful in areas well beyond municipal management, particularly the chapters dealing with governmental use of computers and with intergovernmental relations. John K. Parker's chapter on computers is a solid introduction to how computers work and to their special uses in government; and the chapter on Intergovernmental Relations by William E. Besuden and William G. Colman—the concluding paper in the book—

can stand independently as an essay on the crisis in federalism and on what it portends for the future of cities.

Despite its comprehensiveness and its consistent attention to the larger forces that impinge upon management, the volume has one critical deficiency. This is its failure to deal more explicitly with essential political relationships. The mayor as manager, in contrast with the city manager as manager, is almost completely ignored. One is left wondering where the work of the manager ends and that of the mayor begins. At a time of increasing politicization, this is an important omission. In the same way, the explicit attention given citizen participation in decision-making is also weak. Also, the volume would have been strengthened by including a chapter specifically devoted to new forms of intermunicipal cooperation and metropolitan government. The subject is dealt with as part of other chapters, but given its increasing importance, it deserves attention in its own right. Serious as they are, however, these deficiencies are not fatal, because almost every chapter has provocative observations that relate implicitly to these other concerns.

Because it seeks so broad an objective this volume is certain to invite criticism from many quarters. Those who want a training manual that stays close to the hard details of the workaday world will find it perhaps a bit too analytical and philosophical. Those who see modern management as more art than science may regard those portions detailing the impact of the behavioral sciences on administration as too academic, while specialists in the functional areas of administration may find the chapters of special interest to them on the thin side.

Such criticisms notwithstanding, and despite omissions and unevenness, this volume achieves an impressive overall objectivity and balance. Banovetz and the other authors have produced an excellent introduction to managing the city in today's complex and troubled world.

ARTHUR NAFTALIN

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Industrial Democracy: The Sociology of Participation. By Paul Blumberg. (New York: Schocken Books, 1969. Pp. 278. $6.00.)

The American Idea of Industrial Democracy, 1865-1965. By Milton Derber. (Urbana: University of Illinois Press, 1970. Pp. 553. $9.50.)

A resurgence of political scientists' interest in industrial democracy has been sparked by Peter Bachrach's critique of Robert Dahl and the "democratic elitists" partly on grounds of excluding "private government" from the sphere of democratic theory (*Theory of Democratic Elitism,* Little, Brown and Co., 1967) and by Dahl's more recent attempts to incorporate this criticism within his perspective (in the *New York Review of Books,* Nov. 19, 1970, and in his recent text, *After the Revolution?,* Yale Univ. Press, 1970). As yet, however, basic research in industrial democracy derives primarily from social psychology, sociology, and history in spite of its direct and crucial relevance to such important areas of political science as democratic theory, administrative and organization theory, and interest group theory.

Political scientists with these interests should regard Paul Blumberg's book *Industrial Democracy,* as compulsory reading. It represents an important synthesis of the debate within this field and constitutes a good introduction for those new to it. By contrast, Milton Derber's *The American Idea of Industrial Democracy* obscures many of the theoretical issues in an otherwise valuable historical survey.

With regard to democratic theory, Blumberg's lengthy critique (Chapter 7) of Hugh Clegg's *A New Approach to Industrial Democracy* (1960) raises a number of theoretical issues not adequately treated by Derber. Blumberg begins by asking if there is a sufficient analogy between trade unions and political parties, as asserted by Clegg and by Derber (p. 17), to permit valid comparisons between the contemporary collective bargaining system and political democracy. Blumberg argues that democracy hinges crucially not on the existence of an opposition such as a party or union (he notes the contrary example of town meetings, cooperatives, and the like) but rather on the accountability of a leadership to an electorate which has power to remove and replace that leadership.

Disagreeing on this issue, Derber goes on to define democracy largely in terms of having interest organizations bargaining with a "private government" permanently in power under conditions of equal rights, free speech, due process, access to information, etc. He thus equates the rise of industrial democracy with the increasing legitimation and widening scope of bargaining, obscuring the question of the power of workers relative to employers and the question of accountability. Political scientists will be disappointed in the neglect of process-related aspects like power and accountability in favor of institutional aspects having to do with organizational legitimation under conditions of civil liberties.

Moreover, although Derber likens unions to parties with the exception that conflict centers on sharing power rather than replacing one regime by another, his subsequent historical treatment makes clear that the image thus evoked of a "coalition government" in which different interests or parties share power is also a poor description of the present bargaining system. In fact, Derber concludes that in its eventual mature state, as in the garment industry, the bargaining system involves no challenge to major employer functions, much less the sharing of these powers (pp. 404–5).

By focusing on participation as a process dependent on the distribution of power within a unit rather than upon formal structure and rights, Blumberg's study also tends to be more relevant to a second concern of political scientists, namely, organizational theory and administration. In a lengthy review (Chapters 5 and 6) Blumberg explores the literature of the sociology of participation and, like other authors, finds that it strikingly supports the view that greater worker participation significantly enhances satisfaction and productivity. In contrast, Derber argues (pp. 527–28) that participation studies give "a very mixed picture," although he cites no studies as evidence for this assertion.

This disagreement results in part from a number of conceptual distinctions not always clearly made. First, participation must be distinguished from concern ("the Hawthorne effect"). Blumberg demonstrates how the germinal Hawthorne studies introduced participation as an unintended and unrecognized variable in the name of controlling for a constant level of cooperation, and how morale and productivity rose and fell with participation (Chapters 2 and 3). Since experimenters' concern also rose and fell with participation, however, the relative importance of the two variables remains undetermined. Conclusions drawn by subsequent interpreters were incautious, emphasizing the role of management concern rather than worker participation, although later studies showed the independent effect of participation.

Second, the participation-nonparticipation dimension must be separated from the centralization-autonomy dimension. James Price has cogently argued (*Organizational Effectiveness*, Homewood, Illinois: Irwin-Dorsey, 1968, pp. 82–7) that studies demonstrating the value of centralization to productivity may be consistent with the participation studies and hence centralization studies cannot be used validly as negative evidence for the participation hypothesis. Unfortunately, however, Blumberg damages his argument unnecessarily by equating participation and autonomy. The issue is neglected entirely in Derber in favor of the conclusion (p. 521) that "The collective bargaining model triumphed over its rivals (. . . socialism, syndicalism, employee representation) because it worked better in the American environment than they did and was more congenial in ideology to national sentiment." This sweeping conclusion neatly obscures the criteria by which collective bargaining "worked better" than various participation schemes (effectiveness? satisfaction?) and avoids presenting evidence directly on organizational theory hypotheses. Instead, Derber seems to treat the historical prevalence of the present system as the implicit criterion for "working better."

The introductory nature of Blumberg's book points up the need for further investigation. For example, investigation of the participation hypotheses by each of the nine types of participation set forth in Blumberg's study (p. 71) would be desirable. Due to the paucity of studies in many of these categories, Blumberg has surveyed the field in terms of the more arbitrary criterion of type of research (field experiments, survey research, etc.). Second, there is a need to investigate the importance of participation in comparison with other factors affecting satisfaction and productivity and to establish trade-offs among them. Third, there is a need to relate more clearly the success and failure of workers' participation or control to structural and cultural factors (for example, the role of nationalization or of the kibbutz community). Fourth, further research and analysis should emphasize workers' control as a strategy for labor, as opposed to the managerial orientation of most participation studies.

G. David Garson

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A Single Society: Alternatives to Urban Apartheid. By Donald Canty. (New York: Praeger Publishers, 1969. Pp. 181. $2.45.)

The Political Economy of the Black Ghetto. By William K. Tabb. (New York: W. W. Norton & Company, Inc., 1970. Pp. 152. $5.95.)

To the growing list of contemporary writings on race relations in urban America have been added two more publications. Each is focused on the ghettoization of Black Americans. Each is concerned with how the separation came about. Each is an impassioned plea for a change of that condition. Yet they differ fundamentally in perspective.

Canty asserts that *de facto apartheid* exists in urban America. He expresses shock that the present conditions were permitted to grow because of ignorance on the part of white Americans. "It is astounding to me how quiet the American conscience can be when not prodded," he muses (p. 4). His aim in writing the book is to prod. His thesis is that white Americans must want to do more than they have done and must draw up a massive national plan to build a single society. The adoption of that plan would provide the alternative to urban *apartheid.*

Canty asserts that the practitioners of political power in America have tried to deal with the problems facing the urban blacks, yet were not successful in their endeavors. He traces the history of the "Near-Great Society" programs, and analyzes the many factors that have contributed to their overall failure: fragmented programs, lack of centralized leadership, diffusion of responsibility in the federal system, changing top level administrators and bureaucratic personnel, and a conflicting system of interest groups and political parties.

Tabb's concern is with the racism that "is perpetuated by elements of oppression within an economic and political system" (p. vii). His perspective is "development economics," and his model is the colonial analogy: The ghetto is to white America, as the third-world nations are to the industrial advanced countries. He sets out the essential elements of his theory: (1) economic control and exploitation (by white "outsiders") and (2) political dependence and subjugation. Indeed, he says (p. 21), "In its relation with the dominant white society, the black ghetto stands as a unit apart, an internal colony exploited in a systematic fashion."

Utilizing the "internal colonial" model, Tabb discusses why blacks cannot own the economic resources of the ghetto; how options open to blacks within the capitalistic market system are limited; how programs such as welfare and low-income housing are being imposed and perpetuated; how social welfare policies are sustained as functions of colonial administration; and how the labor market imposes a colonial status on blacks in America.

Tabb writes that among the realities in contemporary America are: (1) Black exploitation, which exists because important segments of white society profit from such an arrangement; and (2) resistance to progressive reform by vested interests whose influence is based on their economic power. Thus, it is the "wealthy" few that are to be blamed for these realities. Since basic policy changes would hit their collective purse (and are to be avoided) they see that only minor programs are enacted from time to time to assist the blacks. Yet such minor programs are "palliatives to calm, not programs for change" (p. 130). As for the mass of white American citizens, they are educated to accept the policies of exploitation for "those who profit from them convince us we need them" (p. 138).

Since national priorities are structured so as to preclude meaningful change, Tabb concluded that economic and political reforms are needed. What should whites do? "First they should support those blacks who are on the cutting edge of the struggle, the militant groups that refuse to go slow and accept the token reforms. Second they should build a white movement for radical social change" (p. 145). In addition, the "liberal and radical whites must stop finding their own political fulfillment in supporting blacks and start to identify the causes of their own problems" (p. 145); and the "overtaxed" white working class should ally with those "excluded from the bounty of society" in a common cause "in reorienting the economy and imposing new social relationships on society" (p. 146).

Canty is a bit more specific and far-ranging in his suggestions about what to do. He seeks a new national *policy,* for America, he says, has already had too many *programs.* He proposes an entirely new system of urban development: Start in virgin territories of the country, and build new cities that would permit coherent growth rather than piecemeal proliferation.

In the meantime, to those who are concerned about immediate change, Canty offers some "starting points." Three tasks can be accomplished: (1) Improve the everyday environment (to alleviate the assaults on the spirit); (2) create additional community service jobs, with built-in training programs (to teach salable skills and to create new careers); and (3) restore law and order in the cities (to eliminate fear and the forms of social distress), and return justice to judicial procedures.

These tasks, long-range or interim, would require the concerted effort of the national government and the large expenditure of national funds. The money would go to the states and localities with a "couple of big strings'" attached, to insure that projects were undertaken; and the national government would be responsible for overseeing the successful implementation of the policy.

Thus, the fundamental difference between the two authors can be seen. Canty's average white American is blissfully ignorant; Tabb's average white American is duped. Canty's politicians flounder because of complexities of political, psychological, and social realities; Tabb's set out systematically to exploit blacks for economic gains. Canty prefers to work within the system, to change programs, priorities, etc.; Tabb advocates a drastic change of the system (for to work within it, he believes, means maintaining colonialism).

Both books are polemics. But in these two can be found the rhetoric of today. Which is better? Which is worthwhile? Which is more persuasive? It depends. If you believe that reason shall triumph once men of good will are awakened, then Canty is reassuring; if you believe that there is an economic conspiracy afoot in America, then Tabb is reinforcing. It depends on whether you can accept Canty's premise that men are blind to the problems of others whom they do not see in their daily lives; or Tabb's proposition that white America has perpetrated a carefully designed and executed colonial policy. Canty's is an evaluation of a complex system of policy making; Tabb's is a simplistic explanation of the unseen-elite, economic conspiracy theory. Both will stir the emotions of the reader; neither will persuade intellectually.

MORRIS LEVITT

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State Department, Press, and Pressure Groups: A Role Analysis. By William O. Chittick. (New York: John Wiley & Sons, 1970. Pp. 373. $9.95.)

Professor Chittick's book is a useful addition to the literature on governmental communication if for no other reason than the topicality of its subject matter. Virtually everyone these days, so it seems, is concerned with the relationship among government, press, and public. The expressions "credibility gap," "news management," etc., have become all too commonplace in our everyday language.

Aside from its relevance, Chittick's book is welcome because it is scholarly. He is attempting to refine, replicate, and, where necessary, refute the previous work done in the field of foreign policy opinion formation. Comparisons with Bernard Cohen's *The Press and Foreign Policy* are especially inevitable. Cohen's work was largely the product of reflection and qualitative judgment. Chittick's study rests on detailed attention to methodology and data. Together they are nicely complementary.

Unlike Cohen, however, Chittick is not primarily interested in just the role of the press. Instead he is concerned with four elite groups that supposedly constitute a distinct political system within the overall "opinion-policy process." The relevant elites are government policy officers, government information officers, foreign affairs reporters, and leaders of nongovernmental organizations (*i.e.*, pressure groups). Intuition and conventional wisdom suggest that conflicting interests among these groups pose difficulties for the democratic management of foreign affairs. State Department officers are oriented toward effective policy-making, which may require secrecy, speed, and so on; reporters, and NGO leaders are interested in public opinion formation, which assumes the dissemination of information and at least some encouragement of popular participation. Through the use of role analysis and systematically structured interview data, Chittick attempts to determine how the content of both policy and opinion is affected by these groups, and, more importantly from a theoretical standpoint, how their mutual attitudes, expectations, and perceived antagonisms reflect whatever incompatibilities may exist between the requirements of democracy and successful diplomacy.

Not unexpectedly, Chittick's findings are rather pedestrian. Antagonism *is* perceived among his various "counter position holders," and it fluctuates within each group according to any number of variables. The most "salient" conflict is that between State Department policy officers and foreign affairs reporters. Interestingly, in this particular relationship, the degree of perceived antagonism varies considerably with positional factors—that is, the greater the policy responsibilities of the State Department officer or the interpretative pressures imposed upon the reporter, the more likely are these individuals to be antagonistic in their perceived relationships. This finding is of some significance since it runs contrary to Cohen's emphasis on personality factors (*e.g.*, self-confidence), and it supports the notion that such conflict is inherent in a democracy's handling of foreign affairs.

The real value of Chittick's book rests not with its conclusions but with its attention to detail and method. The author has interviewed more than 150 individuals and has treated their responses in appropriate statistical fashion. In his 65 pages of appendices, he has included his interview schedules, some selected findings, and a meticulously thought-through research design. In the body of the book he has called attention to proper distinctions previously unemphasized. His chapter on the "Characteristics of Position Holders," for instance, differentiates between the needs of varying kinds of foreign affairs reporters (*i.e.*, wire service *vs.* television and radio), the differing policy interests of nongovernmental organizations, and the often conflicting role requirements of State Department policy officers and information officers. In the latter case, unfortunately, Chittick does not emphasize enough the negative nature of his findings. His data suggest that there is relatively little perceived antagonism between the policy officer and the information officer. This is not as it should be, since the information officer has a monitor's as well as a broker's role to play. If he is encouraged to perform both tasks aggressively, his relations with his policy colleagues should reflect a greater degree of antagonism than is apparently the case. Such conflict ought to prove functional from both the opinion-forming and the policy-making perspectives.

Obviously, the conflict that does occur among State Department officials, reporters, and NGO leaders is of a mixed motive variety involving strong elements of cooperative as well as competitive interests. This probably explains why Chittick's data are often not very impressive. The relationships among his variables are seldom very strong, and he finds himself facing serious exceptions to certain of the things he no doubt would like to emphasize (for example, positional factors appear only minimally important in the perceived antagonism between State Department officials and NGO leaders). Occasionally, he ignores the weakness of the data and goes ahead anyway. Thus he notes that "information officers appear to be more likely than policy officers to perceive antagonism in their relations" (p. 286). This conclusion is based upon a one-tenth of a point difference on a six point scale (1.4 *vs.* 1.3—p. 285). It would seem that the proper evaluation here is that any antagonism that exists is perceived equally by both groups.

Stylistically, the book is well organized, but occasionally it trips over the tediousness of its own data. One gets the impression that the material could have been covered more judiciously in 75 pages less—a common complaint, I suppose. As previously noted, Professor Chittick's conclusions are not very profound (*e.g.*, conflict *is* inherent in the democratic management of foreign affairs, but at least it is out in the open and consequently may prove functional in the long run). This is no reason for not having undertaken the study, of course, but it may be sufficient to move the book down the list of "must read" priorities for the rest of us. It is a best buy, however, if one is interested in conscientious effort and careful methodology. Furthermore, State Department decision makers are still a relatively untapped source of interaction data, and we should be grateful for all the information of this kind that becomes available.

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Politics of Pollution. By J. Clarence Davies III. (New York: Pegasus, 1970. Pp. 231. $6.00.)

Politics of Pollution is an important exception to political scientists' lack of attention to environmental politics. With notable clarity, Davies introduces the reader to "how governmental pollution policy is made" and "the interests and ideas competing for dominance over pollution control." He attempts to integrate the concepts and questions of a political scientist with a working knowledge of the policy process in Washington, D.C. Now on the staff of the Council on Environmental Quality, Davies was formerly Examiner for Environmental and Consumer Protection in the Bureau of the Budget (1965–1967) and Assistant Professor of Politics and Public Affairs at Princeton (1967–1969).

Aside from Lynton Caldwell, no political scientist has achieved professional visibility from research related to the environment. For example, despite an extensive literature search, Davies uses only three minor articles by political scientists on matters of pollution politics. Davies observes that "Everyone opposes pollution yet we find pollution everywhere . . . and the explanation for the gap between intention and reality lies to a great extent in the realm of politics." Yet political scientists have provided few explanations for or guidelines to control pollution. And of all the "issues" of the environment, pollution is the most critical, for it may determine man's very survival. Therefore, the book is a timely description and analysis of a first-rank policy problem.

The book is divided into three parts: first, the pollution problem and growth of public interest and passage of federal legislation; second, the forces shaping pollution policy; and third, major policy processes in controlling pollution—research, standard-setting, and compliance. Davies elects to stay close to Washington politics in citing or detailing specific bills and conflicts. He avoids the muckraker style—the book is not in the tradition of Rienows' *Moment in the Sun* or Ridgeway's *Politics of Ecology*. Rather than excoriate the bad guys, Davies examines the political context and rules of the game within which pollution control policies are formulated, adopted, and implemented. I was especially impressed by Davies' fair and balanced interpretations of the varied issues which underlie and bedevil the control of pollution. (It should be noted that the book focuses primarily on air and water pollution and not on such related subjects as pesticides, radiation, noise, and solid waste.)

In the introduction, Senator Edmund Muskie justly commends Davies for the excellence of his guide to the politics of pollution. Davies does *not* examine the nature, causes, extent, or effects of pollution, the state of control technology, the relationship of pollution with other environmental problems, or the programs necessary for actual control. Rather, Davies skillfully escorts the reader through such questions as why pollution occurs, how it is defined by the polity, how control priorities are set, what federal control policies have been adopted, and what conflicting pressures and views exist in the legislative, executive, administrative, and state and local policy arenas; he also examines the politicized character of research, standard-setting, and compliance procedures. Though the book was written for a general audience, various observations should prove useful for those with research interests in pollution politics. For example, Davies says that the effectiveness of compliance procedures depends on four major factors: the nature of the regulating agency, the cost of abatement, the sensitivity of polluter to public opinion, and the political influence of the polluter. While these formulations are perhaps not theoretically startling, Davies develops many such explanations that should assist, and even encourage, research on pollution control.

While ably sorting out the political issues of pollution control, the book is in substance an introductory text, primarily descriptive and reportorial. Moreover, because its examples, information, and recommendations are tied closely to specific historical circumstances, the book will quickly lose much of its classroom value and public appeal. (Within a year of publication, we have seen the adoption of the three changes Davies says are necessary for more effective pollution control—namely, the creation of a federal environmental agency to encompass and coordinate all control activities; federal laws to facilitate prompt "voluntary compliance"; and a distinction between health and environmental standards, at least in air pollution.) Further, Davies demonstrates minimal concern for questions of theory or method. Aside from saying he will "use an amalgam of rather orthodox political science concepts," he does not discuss his approach to politics or his research choices or sources. He evidently immersed himself in the pollution control literature and related governmental documents, conducted some unstructured interviews, and drew upon his experiences and exposure to Washington politics. Political scientists will be disappointed with his highly selective use of and general disinterest in the available research on legislation, administration, and policy. Inside dopesters will also be disappointed, for Davies is cautious, even bland in his analysis of the political players and past legislation. To take one example, while most observers strongly criticize the Air Quality Act of 1967 for enacting symbolic rather than realistic control procedures, Davies prefers to comment on the Act's mandate rather than to assess its promise. And environmental activists will be dissatisfied by Davies' lack of urgency and preference for modest reforms—he talks mainly about tinkering with the legislative criteria and the organizational mode of pollution control.

As the first major attempt to apply the concepts of political science to pollution politics, the book offers an excellent descriptive and integrative commentary. Unfortunately, it contains little in the way of systematic analysis or original research. If pollution is a "crisis" problem and the solutions largely political, then political scientists have a special obligation not only to comment on but also to investigate the applied questions of pollution control. Despite its limitations, *Politics of Pollution* is an important beginning, and as such, it warrants a careful reading by those who teach or do research on environmental politics.

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The Rediscovery of Black Nationalism. By Theodore Draper. (New York: Viking Press, 1970. Pp. 211. $5.95.)

The author discusses the antecedents, the historic roots of the resurgent black nationalism of the 1960's. The volume is intended for the general public and those whose scholarly interests never extended to the intellectual constructs of black American leaders. Yet, scholar and layman can find something of value in this book.

Historically, black nationalism in the United States has taken two basic forms: "migrationism" or "emigrationism"—that is, external migration—and "internal statism" or self-determination. White Americans have contributed significantly to the generation of both forms of these ideas. An early example was Thomas Jefferson's advocacy of colonizing emancipated slaves in Africa or some other faraway places. Though a slave owner, Jefferson regarded slavery as a "great political and moral evil." Jefferson submitted to the Virginia Assembly a plan for freeing all slaves and arranging for them to be resettled in some other country. A half century later Abraham Lincoln found himself in the same dilemma—strongly opposed to slavery but, being unable to accept Negroes as equals, believing that abolition of slavery should be followed by colonization of freedmen in another land.

Draper examines colonization movements of the 19th century and the roles of black and white leaders. He shows that the pre-Civil War white leadership hoped to foist African nationality upon American Negroes for the same reason that caused later blacks to want to give up American nationality: the assumption that blacks and whites could not live together in mutual respect and equality. Despite continuing efforts to convince Negroes of the desirability of emigrating to Mother Africa or the Caribbean Islands, the free black population generally rejected black nationalist overtures, preferring to wage their battle in the land of their birth. This is all the more impressive in view of the efforts of outstanding charismatic black leaders like Martin R. Delany, the "father of black nationalism."

There is a discussion of the Pan African movement as reflected in (1) ideas and efforts of W. E. B. Du Bois in behalf of the Negro cause throughout the world, and (2) Marcus Garvey's dramatic, often tragic quest for Negro nationhood. The author deals with the black nationalism that manifested itself in the idea of self-determination—a call for an independent black state or nation within the United States. In 1917 Cyril V. Briggs, in editorials in the *Amsterdam News,* asked whether "it is not time to consider a separate political existence" by establishment of a "colored autonomous state" in the far western United States. Perhaps the most publicized version of the Negro state idea was advanced by the Communist Party in the late '20's. Beginning with the premise that Afro-Americans constitute a "national question," the Sixth World Congress of the Communist International in 1928 adopted the principle of "the right of self-determination of the Negroes in the Black Belt."

In the North, where the black masses were "working for assimilation," the party made "equal rights" the main slogan. There was much disagreement among Communists about this doctrine, and so the statement left open whether blacks themselves would want to set up an independent Negro state: "Complete right to self-determination includes also the right to governmental separation, but does not necessarily imply that the Negro population should make use of this right in all circumstances, that is, that it must actually separate or attempt to separate the Black Belt from the existing governmental federation with the United States. If it desires to separate, it must be free to do so; but if it prefers to remain federated with the United States it must also be free to do that." Two factors soon demonstrated this doctrine's lack of viability: the blacks' lack of interest in it and the massive migration of Negroes out of the Black Belt region.

Draper devotes chapters to such manifestations of black nationalism as the Nation of Islam (or the Black Muslims), Malcolm X, the Black Panthers, Black Power, and Black Studies. In describing the cosmology and mythology of the Muslims' theoretical system, the author explains why Elijah Muhammad's practical politics have tended to reduce rather than stimulate and exacerbate black-white confrontations. He notes the variety of elements in Muslim black nationalism: ethnological fantasy, theological credulity, autocratic theocracy, internal statism, intense racism, psychological emigrationism, economic separatism, political isolationism, and individual self-improvement.

The author traces Malcolm X's change from loyal follower of Elijah Muhammad to independent leader of his own movement, from American black nationalism to African black internationalism, from physical emigrationism to psychological and cultural emigrationism, and from strident racism to revolutionary internationalism and humanism.

Viewing the Black Panthers as a purely political nationalist movement, Draper describes their impatience with Back-to-Africa nationalists and cultural nationalism manifested in dress—dashikis, bubas, "natural heads," etc. The Panthers have moved toward a social rather than a purely nationalist revolution. As Draper points out, espousing their own brand of Marxism, the Panthers added socialism to nationalism, providing a basis for coalition between black and white revolutionaries to overthrow the common enemy, capitalism.

In identifying the specific contents of "black power," Draper notes its marked success as a slogan and its difficulties as a policy. He looks briefly—all too briefly, perhaps—at "the land question" in black nationalist movements in the United States. He notes the cresting of "integrationist" and "Back-to-Africa" waves in the mid 1960's, the reduction of the African orientation to essentially superficial cultural manifestations (fashions and hair-dos) in the late '60's, the persistence of the demand for land in movements like the Republic of New Africa, and its implications for massive population relocation.

The author considers the impact of black nationalism on academe, giving rise to the demand for Black and Afro-American Studies. The recurrent themes he finds in the ideology of academic black nationalism are racial solidarity, nation building, and self-determination. He concludes that two basic types of Black Studies programs have emerged: (1) Black Studies and (2) Black Nationalist Studies. The latter programs, in his judgment, reflect a rigid conformity to a black-nationalist party line, and are characterized by "onesidedness, discrimination, and exclusion."

Accepting the validity and necessity of Black Studies, Draper insists "that the real question is whether these [studies] should be studied and taught the way other subjects are studied and taught, as a collective educational enterprise, or whether they should be studied and taught with a built-in nationalist mystique outside the bounds of rational discourse and authentic scholarship" (p. 164). Although he takes a hard line on politicized Black Nationalist Studies, he recognizes the intrinsic validity and significance of the subject matter.

The author raises certain questions he regards as crucial: the extent to which black nation building can take place without major collision with the larger American nation-state; the limits within which black people can feel that they are expressing their individuality and controlling their own fate; the appropriate limits of "integration" and "separatism" in a society that is only a little more than ten per cent black and almost ninety per cent white. He believes the answers will not be found in nationalist fantasy, on the part of either blacks or whites. For "what is at stake," he concludes, "is not a black nation or a white nation but an American nation."

The major weakness in this very thoughtful volume is the author's tendency to overgeneralize and failure at times to develop ideas adequately. To state a point is not necessarily to prove it. In places, a calmer appraisal would be helpful. The assumption that racial confrontation is inevitable can contribute to an explosive situation. In any event, this essay makes a significant contribution to the literature dealing with an oppressed people's multifaceted struggle for social justice in a society with a deeply rooted racist tradition.

ROBERT E. MARTIN

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The Sociology and Politics of Congress. By Lewis Anthony Dexter. (Chicago: Rand McNally & Company, 1969. Pp. 300. $4.95 paper.)

Some books can be read with little or no awareness of who the author is, what his frame of reference is, and what biases shape his perspectives. Not so with this book nor, for that matter, almost any of the penetrating and original contributions of Lewis Anthony Dexter. At an annual political science convention about a decade ago someone said to me: "Doing research on Congress? You really ought to meet Lewis Dexter—he's the one authentic, mad genius in our profession." Mad he is not; eccentric maybe. Who else failed his orals or dissertation exams seven times, but nevertheless received his Ph.D. (Sociology, Columbia University, 1960) and lists both facts in his resumé? (A footnote on page 191 of this textbook sheds insight on what must have been some epic encounters: "In 1956, when examined by Paul Lazarsfeld and Herbert Hyman on my work on Congress I insisted that I had learned my 'methodology' from Walpole and from Trollope; I was more serious than not, although they thought me merely perverse.")

The assessment of genius can seldom be made by one's peers; one man's genius may be another man's perversity. Still, it is difficult to work within a subfield like Congress or a broader discipline such as political science without developing rather strong impressions about what constitutes significant contributions. Leaving aside the importance of Dexter's inputs into the book that won the Woodrow Wilson Award in 1963, *American Business and Public Policy,** what continually impresses me about his work is its breadth and originality. His articles (some forty in number) range across such diverse interests as elections and campaigning, civil defense, Congress, social innovation, occupations, mental retardation, theories of perception, transactional analysis, and political philosophy. Almost without exception the dozen or so that I have read either suggest a new way of seeing a problem or provide a better handle on how to research it. And this makes Dexter, at least to this observer, a prime example of the kind of social scientist who is moving or may yet shift the discipline away from its "normal science" preoccupations toward a new and more valuable paradigm.

The Sociology and Politics of Congress illustrates Dexter at both his best and worst. It sets out to answer, and generally succeeds in answering, three major questions:

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* Raymond A. Bauer, Ithiel de Sola Pool, and Lewis Anthony Dexter, *American Business and Public Policy: The Politics of Foreign Trade* (New York: Atherton Press, 1963).

> (1) How far can citizens affect legislative outcomes and actions by the voting choices they make and by the part they take in election campaigns? (2) How can citizens affect legislative outcomes by means other than election activity—such as lobbying? (3) In interpreting and trying to influence Congress, what ideas, conceptions, and images are most useful for what purposes?

While each of the three parts of the book concentrates on one of these issues, more than half of the book is devoted to the first question.

Dexter argues convincingly that elections can make a difference both with regard to specific legislation and the overall climate or mood that characterizes any given Congress. He goes on to caution that few congressmen or senators are ever beaten on a single issue. In common with a great deal of contemporary congressional research (Fenno, Manley, *et al.*), the importance of committees and subcommittees, especially in the House, is stressed. Finally, Dexter shows with abundant examples how the defeat of one or two members on critical subcommittees might drastically alter the shape of legislation in past or future Congresses.

Not surprisingly, many of the examples of how citizens might influence legislation center around reciprocal trade policy. From the author most responsible for the interest group and congressional sections of *American Business and Public Policy,* one could hardly expect otherwise. Still, I found it difficult to separate out what I had absorbed before and what I was discovering afresh in this book. Put another way, the first part of the book is primarily designed for beginning students and action-oriented citizens. The professional observer of lobbying and legislation will benefit more from a return to the original 1963 study.

The last two parts of the book are less useful in the classroom, more valuable to the research-oriented social scientist. For example, I know of no more penetrating treatment of how congressmen react to pressures than what Dexter calls "the fog of policy demands." Confronted with innumerable requests for attention, each congressman, aided by personal and committee staff, comes to concentrate on a limited few. Inevitably, congressmen's attention to most demands must be marginal and peripheral. This basic mode of perception underlies, if it does not account for, most specialization of labor so characteristic of legislative activity, and helps explain the pervasive subcommittee and committee structure in both houses. Dexter's discussion of marginal attention nicely complements the reprinting of his classic article, "The Representative and His District." First published in an applied anthropology journal in 1957, after being rejected by this *Review,* it has appeared in virtually every contemporary congressional reader, in print.

In Part Three, the author's all-too-brief discussion of alternative ways of viewing Congress (five pages), leads into a full-blown discussion of analogies for comparing Congress, not only with other legislatures, but other institutions as well. Why try to explore possible analogies for Congress? Precisely because it forces one, in Dexter's words, "to try to think about what Congress is, what it does that is significant." In a nation entering its second decade of an undeclared war in Southeast Asia and beset at home by problems of social unrest, environmental pollution, crime and poverty, such questions are crucial.

Many strengths of this book have been suggested; it remains to say something of its shortcomings. The principal one is unevenness. Half the time the author seems to be aiming at college freshmen, non-Byrd-watchers and League of Women Voters types; in other places (especially the concluding chapters) Dexter is largely writing for his professional peers. More than eighty pages are devoted to appendices, mostly interview protocols and thumbnail sketches of Congressmen active in the 1950's on the issue of reciprocal trade legislation. Appendix A, demonstrating some of the shortcomings of a too-literal translation of presidential coattail effects, would have been aided by at least a sample of the actual statistics for the 1960's in a state or two where the author's feel for the data would be superbly grounded (Massachusetts, Maryland). The protocols, rich as they usually are, are nevertheless dated. Here again, the overpowering influence of the earlier book pervades this text. Chapters Three and Four, treating of ecological issues, only partially offset this prime emphasis on examples from reciprocal trade and tariff policy.

Still, at the risk of possible self-contradiction, I would nevertheless recommend using this paperback in introductory American government courses as well as graduate courses, allowing individual students to seek their own level. The best ones will have an opportunity to soar very high, indeed.

ROBERT L. PEABODY
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The Federal Courts as a Political System. By Sheldon Goldman and Thomas P. Jahnige. (New York: Harper & Row, Publishers, 1971. Pp. 292. $3.95, paper.)

Traditional jurisprudence was done in by the legal realists and the sociologists a half century ago. It has been the task of political scientists to replace it with an approach which will more satisfactorily organize and explain the data of the jurisprudential world. Several alternatives have been explored. Most readily acceptable was the judicial process approach, which analogized court behavior to that of other political institutions, such as Congress or political parties. This type of analysis stimulated research in judicial biography, judicial socialization, the politics of judicial recruitment, the source of judicial policy preferences, special characteristics of judicial access, and the relations of courts to the other political branches.

A second approach, more restricted in its explanatory scope, was the bureaucratic model of the judiciary in which Weberian concern with rule, norms, and authority patterns was merged with a more recent emphasis on informal organization and role conceptions and expectations. A third type of analysis picked up where the legal realists left off and subjected the decision making of individual justices to study by more or less sophisticated measurement techniques ranging all the way from bloc analysis and cumulative scaling to factor analysis and Boolean algebra. With these new tools political scientists swarmed over the slag piles of traditional jurisprudence and gouged out hundreds of glittering findings about judicial behavior and the judicial process.

Meanwhile, back at the ranch, Eastonian systems analysis was fast winning favor for its generalizing explanatory potential and its capacity to interrelate political events, and for some students its relevance to the judicial field was almost immediately evident. Although there were several moves in this direction, the first book-length utilization of the systems approach to the judiciary was *The Federal Judicial System*, published in 1968 by Thomas P. Jahnige and Sheldon Goldman. They selected from the extensive judicial behavioral and process research a representative array of pieces and fitted them into a systems framework. Though some of the wine was old (*Harvard Law Review*, Louis D. Brandeis, Franklin D. Roosevelt), the labels on the bottles were new, since the selections were grouped under the major systems constructs—input, conversion, output, and feedback. In a brief introductory statement the editors outlined the case for systems theory as an analytical framework to promote understanding of the federal judiciary, but their major contribution was to demonstrate how a mass of uncoordinated research and writing by scholars with widely varying interests and intentions could be integrated and subsumed under systems concepts.

This collection of readings must have been a challenge to the two editors, like Priscilla's to John Alden, to speak for themselves. They have now done so very effectively in *The Federal Courts as a Political System*, which should take its place as an essential resource in introductory American government, constitutional law, and judicial process courses. The structure of the book is quite similar to the earlier volume, but the great difference is that now the authors, while utilizing and digesting the work of the principal researchers in this active field, have been able to develop their own position in an orderly and coherent fashion. They have moreover been able to fill in research gaps. For example, because they are trying to see the system as a whole, the chapter on recruitment takes account not only of judges but also of U.S. district attorneys and the federal bar. Essential information of the traditional sort is not neglected, but it may turn up under new headings. Thus data on jurisdiction, appellate procedures, and relations between federal and state courts are contained in the chapter on "regime rules."

The chapter on input brings material into focus which has commonly been overlooked or underemphasized in other studies of the judicial process. Trends in the litigative input to the federal courts are presented statistically, including data on disposition of cases at the different levels. The major structural inhibitors of demand input—lawyers, financial supporters of potential litigants, law enforcement officials, and judges—act, according to the authors, "as if they were engaged in a vast conspiracy aimed at keeping litigants out of court." But what they are doing is performing the essential systemic role of gatekeeping.

The procedures by which the great majority of all civil suits filed in federal court are settled out of court are examined. In more detail the various methods for disposing of federal criminal cases without trial are considered, with major emphasis on "plea bargaining." The authors find some evidence that the criminal case gatekeeping process is in a state of flux, with the conviction rate and the number of guilty pleas both declining, while convictions after trial are increasing. The explanation suggested for the decline in conviction rate is that an increase in dismissals between arraignment and trial has occurred.

Support is also a system input concept, and analysis of the dimensions of support for the Supreme Court leads the authors to review the principal studies of public opinion and the Court. One interesting table posits the size of the Supreme Court's "politically irrelevant public" by comparing the "no answers" or "don't know" responses in a number of different opinion polls on the Court. They set the politically relevant public as from 70 to 90 per cent of the adult population, but recognize that these figures may seriously overestimate the real percentage of those who hold meaningful opinions about the Court.

In a concluding chapter the authors appraise the judicial systems model according to Karl Deutsch's four tests of success in performing organizing, heuristic, predictive, and mensurative functions. On the first score, they argue that systems analysis clearly has greater organizational power than its competitors, satisfyingly integrating the many otherwise disjoined materials found in legal and judicial studies. Likewise they find it effective in its ability to suggest new lines of system research and in its potential for reinterpreting data already collected. As for prediction and measurement, they are less optimistic, and they conclude that systems analysis does not yet yield a theory of judicial system behavior. This is a pioneering effort, and a valuable demonstration of what systems analysis can contribute in the judicial field.

C. Herman Pritchett
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Unused Power. By Stephen Horn. (Washington, D.C.: The Brookings Institution, 1970. Pp. 285. $7.50.)

Congressional research has begun to yield an impressive list of single committee studies. We know a lot now about House Appropriations (Fenno), House Ways and Means (Manley), Senate Banking and Currency (Bibby), House Education and Labor (Fenno and Munger), and House Public Works (Murphy). But we know considerably less about where these committees place in relation to other committees on a number of dimensions. And we are not very far along in identifying or interrelating key variables. If the major research of the future will aim for explicitly comparative studies, a more modest advance may be had from some conscious sharing, for the researcher's "own" committee, of the variables, measures, and hypotheses generated from other studies. Some such sharing would seem a minimal requirement if all the intensive effort spent is to produce generalizations beyond the one committee. For even when all the committees are "done" (and they could be redone for other congresses, other chairmen, etc.), we may not be much farther along in understanding or explaining congressional behavior.

Stephen Horn's study of the Senate Appropriations Committee is set firmly within the single-committee *genre* and exhibits in sharpest form the strengths and limits of its kind. It combines the author's experience as assistant to committee member Thomas Kuchel, interviews with members and staff in the 89th Congress, and a wide range of other data. It is on the whole a richly researched, thorough, and well balanced study.

The range and depth of research may well be the most impressive feature of the work. Horn analyzes the committee membership for seniority, regional representation, and ideology, and gives detail on the assignment process. He outlines subcommittee structure and influence and the overlap in members between legislative committees and Appropriations subcommittees. Thus seven members of the Senate Appropriations Committee who were either chairmen or ranking minority members of legislative committees served on the parallel Appropriations subcommittees as well.[1] Horn describes committee attitudes and norms. He analyzes subcommittee hearings and constructs a measure of members' interest and influence based on subcommittee participation (pp. 126–129). He traces House-Senate and Senate-Executive interactions. And following the now-established ritual for books on Congress, he offers a final chapter on necessary reform.

The view that emerges is one of a powerful senior committee, with influence distributed by subcommittee, which can hold its own in conference with the House. Norms of seniority, specialization, and nonpartisanship predominate. Although the author questions whether the committee functions as "court of appeals" for agencies after program slashes in the House, much of his evidence suggests that it does. The overlap between legislative committees and Appropriations subcommittees, as the author admits, gives Senate Appropriations members "a strong interest in policy goals and ongoing programs" (p. 83). And he admits later: "Although 'slicing up the pork' may seem to be the order of the day on the committee, the rule . . . is not to be too greedy" (p. 90).

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[1] For example, Richard Russell was chairman of the Armed Services Committee, a member of both the Space and Atomic Energy Committees, and at the same time the chairman of the Appropriations Subcommittee on Defense, and a member of the Appropriations Subcommittee on Military Construction. Leverett Saltonstall was ranking minority member on Armed Services, ranking minority member on the Appropriations Subcommittee on Defense, and a member of the Appropriations Subcommittee on Military Construction.

If the key words are power, pork, and program support, there is a curious disjunction between all this and the normative theme of the title and conclusions. The committee, according to Horn, needs to increase its power in order to oversee and control administration. Horn contends that the committee could be "strengthened" in a number of ways. It could, for example, bring decisions out of the subcommittee for a more unified view of related programs. But this is precisely the way to weaken its present program-based, reciprocally supportive structure. In other words, Richard Russell's view about whether power was used or unused on the committee would in all likelihood differ from Horn's.

What we have, then, is one more committee study and an addition from the Senate side to some major work on appropriations politics. (cf. Fenno, Wildavsky). The contribution is merely additive, however. The study offers no theoretical framework for analysis. It is not concerned with possible interrelations among the committee variables that are described, and it is not concerned with comparison. The obvious opportunities for comparing the two Appropriations committees are followed up only briefly in a discussion of committee integration (pp. 151–153) and who wins what in conference (pp. 159–162).

A more serious case of analysis-shrugging comes from the concluding diagnosis of the committee's "unused power." The author argues, as follows:

> the committee needs more information and staff assistance;
>
> its subcommittee structure prevents an overview of related programs;
>
> the Senate (here switching from the committee) is unwilling and unable to inform itself of material necessary to evaluate committee decisions;
>
> the committee has neglected its possibilities for oversight.

But these "ills"—possibly endemic—are spread throughout the Congress and have been widely noted by congressional critics for some three decades. They are by no means unique to the Senate Appropriations Committee and hardly require six chapters of data as a basis for inference. Indeed little use is made in that concluding analysis of the specific findings generated earlier in the study.

All in all, the book does not pretend to be more than it is—a thoroughly researched study of a single committee. While a broader perspective on congressional committees would have served to qualify and enrich some of its findings, the book still makes a solid contribution to the literature.

But, at this rate, we still have some thirty or more committees to go. . . .

BARBARA HINCKLEY

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The Regulators: Watchdog Agencies and the Public Interest. By Louis M. Kohlmeier, Jr. (New York: Harper & Row, 1969. Pp. 339. $3.50.)

As a reporter for the *Wall Street Journal,* Louis M. Kohlmeier, Jr. covered the Department of Justice and the independent regulatory agencies for ten years prior to writing *The Regulators.* The book, then, results from a decade of background studies, events observed, and perceptions recorded. It is topped off by some notions as to what is wrong with the regulatory system and suggestions for reform.

A wide range of topics is covered. In the first major section, national regulation is placed in its political environment. Chapters deal with the selection of regulators and the relationships among regulatory agencies, the President, Congress, and the regulated. The section ends with two chapters which develop the book's major theme: regulation has resulted in diminished competition, with adverse consequences for the consumer, essentially restricted choice and high prices.

The largest part of the book pursues the major theme and develops a host of minor ones in the context of a number of substantive areas: transportation, energy, communications, banking, investment, labor relations, and trade.

Finally, there is a survey of efforts aimed at rectifying problems in regulation and a presentation of the author's own suggestions for changes. Essentially, his argument is that the existing system cannot cope with regulatory failures, the undesirable consequences of economic concentration, and the attendant and growing public frustration with those consequences. Comprehensive review and fundamental restructuring are required. He suggests dismemberment of the regulatory commissions, with regulatory authority which acts to lessen competition written off the books altogether. This includes a good chunk of rate regulation, for example. What remains after the adjustment—and it would be considerable for Kohlmeier is not against economic regulation—is to be reallocated among other institutions. The President's role would be expanded considerably, especially by assuming authority that would add to his capacity to carry out responsibilities defined in the Full Employment Act.

Executive departments would fall heir to some functions. A set of special courts within the federal judiciary would be established to decide certain kinds of cases. The role of the states would be enlarged, in banking and consumer protection especially. All of these changes would be accompanied by vigorous enforcement of antitrust laws.

This summary should indicate that the book is ambitious in scope and in the weight of the message conveyed. Logically, the next question concerns the author's success in carrying out such an ambitious task. The answer depends upon the group he intended to reach, which is not completely clear. Probably it is the mature lay audience, frustrated as consumers, somewhat sophisticated about the workings of the economy, and not entirely opposed to grappling with rather complex administrative and legal questions. If so, the effort is moderately successful. The book is readable; and although its broad scope means that some important areas, such as employer-employee relations, are treated in a very few pages, and others such as agriculture hardly at all, a panoramic view does result. Sharp regulatory vignettes are deftly used to bring points alive.

Scholars will quibble with an interpretation here and an assertion there. The author is unclear—perhaps uncertain is a better term—about the causes of the problems he sets out. At various times it seems to be self-serving politicians, defective legislation, the weight of several decades of incremental adjustment, or goal displacement on the part of the agencies, to mention a few. Perhaps it is all of these. But how they fit together to cause contemporary circumstances does not come through. Indeed there is reason to suspect that the author may have felt differently about the matter at different places in the book, starting with a notion of narrow interests imposing their will through the political process, and ending with a much more complex formulation.

The recommendations for reform, though stimulating, are also open to criticism, not because they are *prima facie* unreasonable, but because a solid analytical base is not laid, and no effort is made to buttress them with specifications as to particular effects and to weigh them. Kohlmeier offers a policy on corporate concentration that would cause fundamental alterations in the economy. True, consumer choice might be expanded and price advantages derived, as asserted, but there might conceivably be some problems or negative effects that people would want to consider. In general, a set of options is offered, but no compelling case for those particular ones is made.

One leaves the book with a feeling of mild discomfort. Perhaps it is because it purports to deal seriously with knotty administrative and legal questions, and a scholarly gloss is applied to underscore seriousness and to suggest analysis in depth. But little pertinent work of political scientists and legal scholars—and there is a great deal—was brought explicitly to bear. And it could have been employed quite beneficially in various places without making the book too arcane for the general reader. The result might have been a better book in terms of contributing to understanding the regulatory process. As it is, little is conveyed in the way of new insights. Observations are dulled and perhaps even distorted by the attempt to relate them to profound questions of a constitutional nature. If Kohlmeier had forgotten about the grand critique and had come forth with an account of regulatory politics and personalities in the context of some interesting situations and cases, as seen by a very perceptive reporter, we would all have been the better for it.

DAVID M. WELBORN

University of Kansas

White Racism: A Psychohistory. By Joel Kovel. (New York: Vintage Books, 1971. Pp. 295. $1.95.)

The White Savage: Racial Fantasies in the Postbellum South. By Lawrence J. Friedman. (Englewood Cliffs, N.J.: Prentice-Hall, 1970. Pp. 177. $5.95, cloth; $2.45, paper.)

Early in the 20th century, in *The Souls of Black Folk* (1903), W. E. B. Du Bois forewarned that the major issue of the century would be "the problem of color—the relation of the darker to the lighter men in Asia and Africa, in America, and in the islands of the sea." The worldwide racial explosions of recent years are confirming his apprehensions; for the racial aspects of politics, once muted or conveniently ignored, clearly emerge as a fundamental (if not the crucial) factor of politics in multiracial societies. Black-white issues plague America; black-brown-white confrontations beset the Caribbean (and, shortly, in all likelihood, Pacific) areas; black nationalism precipitates the exodus of Indians from Africa to England, exacerbating, in turn, racial tensions there; and, apprehensively, both the rest of the world and South Africa fear/await a potential racial conflagration in that nation.

"White racism"—and particularly the European's treatment of native and nonwhite in other societies—has been studied fairly extensively. The vehemence directed against white racism, however, often obscures the broader reality of color and politics—namely, that racism is a worldwide phenomenon. It is not limited to white treatment of nonwhite. Rather, racial confrontations involving people of different colors (and shades of color) are worldwide, precipitating animosities in—to name but a few countries—Indonesia, Malaysia, Pakistan, the Sudan, Sierra Leone, Guyana, Trinidad, as well as the United States and England. Russia, too, has been known to discriminate against visiting black students; and China, in its effort to win support in Africa, has used the color issue to its advantage *vis-à-vis* the U.S. and the Soviet Union.

Studies of white racism often suffer from excessive emotionalism or self-flagellation, prompted usually by the guilt feelings of the observers. There may be some therapeutic value to such studies, but the lack of objectivity, instead of clarifying, often obscures or distorts critical analyses of the "how" and "why" of racism. As a consequence, we really know little about racism, be it the racism of white, black, brown, or yellow societies. What needs to be probed rigorously and objectively are the patterns and processes by which individuals of one color dominate, manipulate and exploit those of another color whom they regard as inferior, for their own power, privilege, or both.

Both Kovel and Friedman are concerned with white racism in America. Kovel's "psychohistory," drawing upon psychoanalytic theory, goes for the jugular. Racism, he argues, is endemic to white Americans, for "it is generated by their culture." He traces racism to the very roots of western culture, concluding that with its emphasis on property, power, and control over things and objects, it embodies a "maladaptive pathology" that is peculiarly amenable to racism. The "normal variant" of western man, he suggests, is the "obsessive-compulsive, or anal personality," who by definition is obsessively punctual, orderly, neat, clean, rational, aloof, and dominative.

Western man, Kovel states, thinks basically in terms of things and objects, not people. Property, because it is something man can possess and control, is highly esteemed. Fearful of his id, or instinctual urges (which he perceives as dark, foreboding, irrational and demonic), western man represses and controls these "irrational" forces. Moreover, he translates these irrational forces into white-black terms: white is pure, good, rational, and controllable; black is dirty, evil, irrational, and uncontrollable. Such notions, Kovel contends, are subsequently transferred to human beings and human behavior. The black man, consequently, is perceived as dirty, evil, irrational (and "sub-human," since rational is seen as "human"); he must therefore be controlled.

In pursuing this analysis, Kovel ignores both historical and sociological factors. Racism, for him, results from the emergence of a particular personality type. His analysis embraces *all* Europeans, but his examples and illustrations, limited as they are, focus essentially on Anglo racism. Thus Kovel ignores (and would reject as meaningless) differences in French, Dutch, Portuguese, and Spanish—as contrasted with Anglo—racism. Kovel sees racism not as the interaction of particular predispositions (values, attitudes, etc.) with particular historical and sociological events and factors. Rather, he views it as endemic to that "western" obsessive-compulsive personality. Historical forces are, in reality, inconsequential. Indeed, Kovel's "psychohistory," as traditional analytic theory (and much of systems analysis, as well), is essentially ahistorical, if not antihistorical.

Kovel's analysis is, basically, a sweeping indictment of western culture, somewhat comparable to the indictments of Norman O. Brown and Herbert Marcuse, among others. All are critical of the emphasis on property, the scientific-technological thrust of western society, and the emphasis on rational thought, order and system. Indeed, it could be argued that Kovel is not really concerned with racism itself. Rather, he *uses* racism as a whipping stick against a western culture which he wishes to purify.

Kovel accuses the early Abolitionists (and especially the Reverend Theodore Parker) of "wallowing. . . in a sense of grandiose guilt," using abolition simply as a means of purifying themselves. Abolitionism, in his view, was "a moral flagellation which both appeases the guilt of the white and patronizes the black. In effect, it exhorts the white to purify himself for higher conquest" (p. 28). Kovel, it could be argued, in *his* search for white redemption has unwittingly assumed the mantle of a contemporary, secular Rev. Parker. But the book should not be dismissed lightly, for it constitutes one of the few attempts made to probe white racism's roots.

Friedman's allusions to "white savages" and "racial fantasies" suggests a psychological analysis, but that is not his major concern. Rather, his central interest is the subjective racial world of *postbellum* white Southern leadership. He clearly develops his thesis that whites, fearful of demonic or irrational "savage" feelings within themselves, projected onto blacks those "savage" attributes. He does, though, explode the myths of "racial liberalism" of selected

Southern intellectuals, writers, and political leaders, clearly showing that their "liberal" views were, in most instances, simply political devices for maintaining white hegemony in a new political context. Through such fantasies of the black as "savage," Friedman suggests, the white man protected the image of himself as rational and more "civilized," thereby rationalizing the perpetuation in new forms of his power and privilege and his subordination of the Afro-American.

The study of race and politics is—or should be, as Du Bois concluded—of crucial concern. Yet it has not been. A few institutes and centers (for instance: at the University of Denver; the University of California at Berkeley; the University of Sussex and the Institute of Race Relations, in England; the University of Paris; and the University of Nice) are beginning to examine the broader dimensions of politics in multiracial societies, but most studies, focusing on specific societies, are still of a fragmented, limited nature. There are few systematic or comparative appraisals of the broader, worldwide dimensions of racism. We need comparative configurative studies that will uncover the roots of racism; that will appraise the politico-historical developments of racism within societies; and that will analyze the broader (i.e., the psychosocial, political and transactional) dimensions of contemporary racial politics. All of which suggests that, despite the years of research and analysis, we still know very little about politics and race.

DONALD G. BAKER
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The Heathen Chinee, A Study of American Attitudes Toward China, 1890–1905. By Robert McClellan. (Columbus: Ohio State University Press, Pp. 272. $10.00.)

The author has assembled a great mass of fairly familiar materials which illustrate how Americans spoke and wrote about China around the turn of this century. They indicate how strong and pervasive the attitudes of contempt and hostility were in the years when China was brought close to its nadir as a helpless victim of Western power, and the United States itself was jangling its spurs as a fledgling imperialist freebooter among its older, more experienced European friends and allies and rivals. In China this was a time when foreign armed force imposed itself at will. In the United States it was a time when bigotry and mob violence were similarly inflicted on helpless Chinese immigrants. This was an important chapter in the history of relations between Chinese and Americans. Unfortunately, this author has done little to put it in any coherent perspective, connecting his numerous citations with little more than expressions of a somewhat plaintive and naive cultural relativism. There are many older works on this period which tell us a great deal more about it. The newcomer to the subject, however, will discover here, for all this work's inadequacy, some of the sources of Chinese xenophobia, and some of the reasons why the notions of a long-standing Chinese-American friendship are so essentially mythical.

HAROLD R. ISAACS
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Crime in Urban Society. Edited by Barbara N. McLennan. (New York: The Dunellen Publishing Co., 1970. Pp. 151. $8.95.)

Crime in Urban Society, edited by Barbara N. McLennan, contains seven essays on various facets of crime and law enforcement. Unfortunately, no intellectual glue holds the collection together: the essays, despite the focus suggested by the editor in her Introduction, are diverse and do not complement one another. The urban area either as a cause of crime or as the scene of it receives only passing mention except in one essay and also in Ramsey Clark's brief Foreword. The editor of the book suggests that the "essays are unified in their purpose, in the sense that they all challenge the prevailing misunderstandings in the crime debate and attempt to demonstrate where there are substantial gaps in our understanding and treatment of the problem." It is not certain that they do this, individually or collectively.

The first selection, Dr. Walter Menninger's "The Roots of Urban Crime: A Psychodynamic Perspective," deals rather more with the psychodynamic perspective than with the problem of the urban milieu, although Dr. Menninger does suggest that the environment of crime is urban society. The second essay, "Assessing the Current Crime Wave," by Albert J. Reiss, Jr., suggests that the FBI's crime index is inaccurate, erring on the low side, that recently reported increases in crime may be only the result of better reporting, and that in any event no present theory accounts for so large an increase in crime as the FBI has reported in recent years. The third selection, "The Social Cost of Crime and Crime Prevention," by Simon Rottenberg suggests principally that the social costs of crime and crime prevention are somehow related, and that our relatively low investment in crime prevention reflects a form of "rational" social choice. Rottenberg's model, of course, has nothing to do with either public or private behavior in the real world, divorced as it is from human considerations which cannot be assigned a cost in the kinds of economic equations Rottenberg sets up.

The fourth essay, by Arthur J. Sills, deals with the problems of state governments in fighting "organized crime." Mr. Sills, formerly the Attorney General of New Jersey, suggests that better intelligence and more money might help. The fifth essay, by former Senator Joseph S. Clark, speaks mainly to the need for federal gun-control legislation but does contain some material of a general nature on the enforcement of federal criminal laws. The sixth selection, by Professor David Fellman, reviews the major developments in Supreme Court rules dealing with the rights of criminal defendants.

The final essay in this volume, "Public Policy and the Control of Crime," by Barbara N. McLennan and Kenneth McLennan, deals with the extent of urban crime, cites some of the demographic data on crime developed by the President's Commission on Law Enforcement and Administration of Justice, suggests that low incomes and substandard housing may have something to do with crime, and proposes the legalization of some of the minor vices, a more rational penal policy, and a statute making mandatory the locking of automobiles.

Although one can quarrel with some of the things in these essays—Professor Rottenberg's analysis, for example, evokes ideological objections—none of them is terribly offensive. Indeed, Professor Fellman's essay is clear and accurate, in a tradition of legal scholarship which occasionally seems to have disappeared. The real problem is that they make an ill-assorted group; unfit for the professional because too elementary, unfit for the student because the editor's assumption that they have something in common sets a bad intellectual example.

ROBERT C. JACOBS

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The Supreme Court and the Uses of History. By Charles A. Miller. (Cambridge: Belknap/Harvard University Press, 1969. Pp. 234. $6.95.)

This is a superb study. It is valuable for both new and old teachers of American Constitutional law and for everyone seriously interested in understanding the role of the Supreme Court in reconciling stability and change. Twentieth-century constitutional issues form the basis of individual chapters illustrating the Court's use of history as an adjudicative principle. Half the book is thus made up of five case studies of Supreme Court opinions.

The Minnesota Mortgage Moratorium case (1934) shows an agreed-upon history of the contracts clause used but then set aside by the majority in an appeal to the principle of an evolving Constitution. In Myers v. U.S. (1926), the postmaster removal case, history pegged largely to the First Congress was warmly embraced by the Court and became its only principle of adjudication. But this was utilizing history that was "neither right nor relevant," and C. J. Taft's majority masterpiece was virtually ignored in later removal decisions. One chapter examines the First Amendment's guarantees of free speech and press, particularly political expression. Here (in matters involving prior restraint, newspaper taxes, picketing, and seditious libel) the trend has been for the Court to free itself from the common-law tradition, and Court-used history has been both relevant and adaptable as a vehicle of judicial reasoning and as a source of contemporary values.

Following a decade of virtual unanimity in civil rights litigation, the Court fell apart in 1964 over the sit-in cases. Miller focuses on Bell v. Maryland to show a major effort by the Supreme Court to deal with a great gap in our constitutional history. Demonstrations in restaurants and other businesses occasioned four different opinions from the bench. The majority reached a decision on nonconstitutional grounds. The three dissenting opinions, however, dealt with constitutional rights, and they did so from different frames of reference through which history was viewed. Douglas refers constantly to "*apartheid*," Goldberg to "places of public accommodation," and Hugo Black to "privately owned establishments." Four methods of handling history were employed: two varieties of history as intent and two species of ongoing history (Corwin's "adaptive interpretation"). This is an instructive chapter in light of the scholarly attention that has been devoted in the last 20 years to the meaning of the Fourteenth Amendment and civil rights laws of the Reconstruction era.

In the 1960's the Court remapped American politics with its apportionment decisions. The role of the judiciary and the basis of representative democracy have been issues that "forced the Court into a most complex confrontation with American history." The author's chapter on the reapportionment cases demonstrates that historical arguments have been uniformly ineffective, but the chapter is very useful because in these cases all varieties of history have been employed by the justices "as a vehicle and as a value of constitutional adjudication."

Miller's five studies proceed from the relatively simple to the most complex in terms of the historical problems presented. The book is rounded out with a theoretical discussion of the basic problem posed by a written Constitution: constitutional intent versus constitutional change. There is a chapter billed as "an exploration in political sociology": it examines the ways in which the justices' use of history in constitutional law is affected by American attitudes toward history, by the Constitution itself, and by the Supreme Court. Miller writes:

> The Constitution is like a patchwork quilt in the care of the Supreme Court. The sources of the quilt are old; they are varied; and individual parts may be changed. But they are all made to fit together; the entity as a whole retains the same outward shape; and, above all, the function of keeping the body politic warm and secure continues to be fulfilled (p. 188).

The last chapter presents both a summary and an evaluation of the uses of history in constitutional law. Miller identifies and classifies the several varieties of history as intent and the more numerous forms of ongoing history. Perhaps the most rewarding lesson to be learned by laymen confused by what the Court does, is an understanding of the two-clause theories. These allow "the Court to restrict some provisions of the Constitution to an historically determined meaning and to expand other provisions to acquire contemporary meaning" (p. 191). Needless to say, a two-clause theory is not a rationale for constitutional change espoused by many members of the Supreme Court. John Marshall's "a constitution intended to endure for ages to come" and the view that law is evolutionary (Holmes' "organic, living institutions") are more palatable reasons for justices to defend intent and constitutional change. Indeed, Marshall's "enduring constitution," set forth in McCulloch v. Maryland (1819) combines both intent and ongoing history. It has the twin virtues of being accepted history (i.e., precedent) and at the same time being an adjudicative principle for solving today's constitutional problems.

This is one of the finest books in the field to appear in recent years. Prof. Miller demonstrates great perception, skillful analysis, and excellent writing.

DAVID FARRELLY
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The Italians in Chicago 1880–1930: A Study in Ethnic Mobility. By Humbert S. Nelli. (New York: Oxford University Press, 1970. Pp. xiii, 300. $8.50.)

There are several points of interest in Professor Nelli's tightly-woven and copiously documented account of the social, cultural, and political life of an important ethnic group—Chicago's Italo-American community. A book that includes in its cast of characters Big Bill Thompson, Big Jim Colosimo, and Scarface Al Capone is blessed with more vitality and cultural flavor than the usual historical monograph. Nelli's study also reminds us that political history can be a means of forgetting the frenzied present, as well as understanding it, and there is something finished and whole in this contribution to the genre of ethnic community studies. Although living in the bustle of a metropolis, Chicago's Italo-Americans remained in many ways villagers tied to the roots of family blood (much like their compatriots in Boston's East End, studied by Herbert Gans).

The most significant aspect of *The Italians in Chicago* is that old-style ethnic politics, by which I mean the study of white, European-derived groups, is now the domain of urban historians rather than of political scientists. The interests in ethnicity that not long ago produced the insights of Holden, Parenti, Wilson, and Banfield (to mention only authors writing on ethnic politics in this *Review*) are not visibly attracting concern among a younger generation of political scientists. Perhaps it is the nature of our politicized times, the attraction to the drama of the Chicano and Afro-American. Perhaps, too, there is no creative tension between one's distant Polish-American or Italo-American past and the assimilated mod world in our present-oriented society. Whatever the reasons, "old-style" ethnic groups are now in the urban historians' domain, and to them we who are interested in these concerns must turn for perspective and reflection.

Unfortunately, Nelli provides a weak intellectual bridge between disciplines and scholarly interests. His main point is that the solidarity of the Italo-American community provided distinct patterns of political behavior and social mobility. He traces the forces that perpetuated this solidarity in family life, immigration, and interestingly enough in Italo-American criminal organization which reinforced control of the areas in which new immigrants settled. He suggests that although Italian crime was part of the American way of life it retarded residential mobility and cultural assimilation within Chicago. He also describes the appeals to Italo-American voters usually in the context of a swing group between the Irish and Eastern European blocs in Chicago politics. Nelli has written a case study, limited in time and place, and consequently he ought not to be held responsible for recent social science scholarship in ethnicity; but I do wish that he gave better evidence of understanding and using such basic concepts as social mobility and residential mobility, assimilation and acculturation, and that he had some familiarity with the literature on political behavior. On the other hand, had he written a broad, interpretive political history, such as Joseph Huthmacher's *Massachusetts People and Politics,* he could have provided intellectual grist for the mills of contemporary social scientists. But such general uses of either social science or interpretive cultural history, alas, are sparse. The present study is narrow and deep, although most suggestive in linking Italo-American crime to group life and social progress. In effect, Nelli has provided us with an account of some of the costs paid for the use of alternative ways to success and power by ethnic group members.

Here, then, are points of interest for urban specialists, specialists in Italo-American life, and devotees of Chicago's lusty past. A few years ago, in *Beyond Pluralism: Ethnic Politics in America,* I observed that the heart of ethnic politics was the clash between native and newcomer in the arenas of politics, values, and cultural life. Today, where might the broad gauge scholar find creative tensions among the older European-derived stock? The perceptive eye does barely notice Secretary of Transportation John Volpe amidst the Middle-American, Waspish echelons of the Nixon administration. Ethnic mobility, indeed, but hardly the stuff of political drama to restore ethnic studies within Political Science. Public administration specialists may find in Volpe, Italo-American politician of national visibility, what discerning ethnicists no longer perceive amidst the social, ideological, and political chains that link the old ethnic politician to his tribe and to the outside world. Assimilated and accepted, Volpe blends unnoticed into the brownish landscape of the Mitchells and the Doles. Consequently, he lacks the social flavor Scarface Al Capone might provide when seated next to Martha Mitchell at a White House dinner. My fantasied scenario goes like this:

> MM: And how do you manage things in Cicero, Mr. Capone?
>
> AC: Well, dolly, I tell them you deliver the gangster vote and I'll back you.
>
> MM: John, aren't we late for the party at the Rodgers?

Or, imagine Volpe's compatriot, "Big Jim" Colosimo (a helluva guy who runs a wet, open town) discussing noble purposes of government with the Rasputin of the White House, the Reverend Billy Graham.

In the final analysis, ethnic politics is that of passion, of blood, of believers, and the watchful tribe probing the strange ways of American life. "Political power in a democracy rests upon friendship," asserts Chicago's Italo-American politician, John Lanseco in Nelli's volume. And on more than a modicum of hatreds, one must add. But it has never been sustained by the bureaucratic style of a Department of Transportation press conference. Consequently, it is doubtful that Secretary Volpe would find any possible political and social uses for the services of his compatriot, John Lanseco. But, then, at least John Lanseco and his constituents in Chicago's Italo-American community had compatriots.

EDGAR LITT

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Political Change In California—Critical Elections and Social Movements, 1890–1966. By Michael Paul Rogin and John L. Shover. (Westport, Connecticut: Greenwood Publishing Corp., 1970. Pp. xviii, 231. $12.50.)

To probe the critical elections which have occurred in the Golden State ever since the Populism of the 1890's is inevitably to learn a lot about California politics—especially when one can capitalize on the scholarly efforts of a first-rate political scientist working in tandem with an equally competent historian. That is the boon conferred on its readers by the Rogin-Shover analysis of *Political Change in California.* Yet their book does not achieve anything like perfect elucidation of what has happened on the California political scene during the past eighty years.

The contents of the volume are presented in six chapters, each supplemented by an unusually comprehensive set of footnotes: (1) California Populism and the "System of 1896," (2) Progressivism and the California Electorate, (3) The Progressives and the Working-Class Vote, (4) Was 1928 a Critical Election in California? (they say No) (5) The Resurgence of the Democratic Party in California, and (6) Southern California: Right-Wing Behavior and Political Symbols. Applying V. O. Key's theory of critical elections, the authors in Chapters 1–5 take the vote counts in state and national elections from 1890 through 1966 and subject them to quantitative analysis that is sufficiently rigorous and minute to bring out the relative importance not only of the ethnicity, religion, and economics but also of regionalism, personality, and ideology. In contrast, the last chapter amounts to a venture in political anthropology. Its thesis is implicit in these two sentences: "Politics is remote from the lives of most people. . . . To understand how people think about politics, and why they vote as they do, we have to know how they live" (p. 181).

More specifically the argument is that climate, topography, economic fortune and misfortune, and the pattern of immigration (not least of Blacks and Chicanos) have combined in southern California to give rise to a way of life that exaggerates its own Gilded Age development beyond that which has occurred in the rest of the country. As a result the WASP majority in the eight southern counties has tended toward right-wing extremism. Observing that "A southern California political tradition has manifested itself with impressive continuity since the 1890s," the authors argue that, because its growth in population now guarantees this region the balance of power in state elections, this tradition will, from here on, be increasingly important to an understanding of the politics of the Golden State.

And so it will. But, perhaps to make their point more forcefully, they indulge occasionally in a bit of caricature instead of restricting themselves to quiet recitations of objective facts. Here are a few examples:

> All that is personal and unique is burned out of the southern California landscape. . . . In theory, the southern California family is a refuge from the competitive, anonymous outside world. But the claim to a family-centered, child-centered existence has a hidden meaning. Family love in fact is not unconditional, but a tool in the fashioning of the child; he learns what he must do to earn it. . . . Indeed, there is no unplanned street life at all; school playgrounds stand empty. . . . The house is less a home than an investment. . . . Boosterism moves from the marketplace into the family (pp. 195–8).

They note, to be sure, that these same tendencies are to some extent present in American middle-class family life everywhere. White southern Californians simply carry them to extremes. But do they? And if they do, will not this also change? Many political happenings in Los Angeles and Orange counties are truly fearful and wonderful to behold. But it should help social scientists hold fast to that priceless thing, a sense of proportion, to remind themselves every now and then that while the voters of Orange County, the very symbol of right-wing "extremism," have been sending a super-conservative like James Utt or John Schmitz to Washington from one of their Congressional Districts, they are represented in the other one by Richard Hanna, a truly responsible liberal.

There is much to recommend the kind of interdisciplinary collaboration which led to the publication of this study. No one of the social sciences is sufficient unto itself. By pooling their talents to produce this incisive and well-written analysis of critical elections and social movements in what has recently become the most populous state in the Union, Rogin and Shover have set an example which it is hoped many others will follow.

JOHN A. VIEG

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American Images of Soviet Foreign Policy. By William Welch. (New Haven: Yale University Press, 1970. Pp. 316. $10.00.)

In a "super-book review," Welch has undertaken to examine and appraise American writing on Soviet foreign policy, dealing with twenty-two works of 1959–1968 and in detail with nine: three "Ultra Hard," Goodman, Wolfe, and Strausz-Hupé; three "Hard," Kennan, Shulman, and Tucker; and three "Mixed," Gehlen, Halle, and Schuman. His purpose is to develop a methodology for the better analysis of Soviet foreign policy, primarily to make it more helpful for policy makers; but much of his critique is pertinent and of broad interest.

By calling attention to bias, unsound methods, and unsupported conclusions, Welch may contribute to an improvement of American writing on the subject. But his goal of bringing together images by a more objective approach is herculean in the confusion of values and uncertainty of facts. How can we judge the intent of actions and words when we know almost nothing of policy debates? How can one determine which statements are self-serving and which self-revealing? Are Soviet goals really Russian or those of a multinational organization? Is the Soviet regime essentially new and *sui generis*, or a continuation of the Old Russia, or a state rather like others? Do Soviet leaders have clear-cut long range purposes? Where is the boundary between legitimate enlargement of influence and improper expansion of control? Is Soviet behavior mostly determined by the international environment or by Soviet needs and character? Touching such questions, Welch cannot be faulted for not answering them.

The key undecidable question is whether the Soviet Union is really expansionist. Answers, as Welch infers, hinge on the question of legitimacy of acquisitions. Are the Soviets behaving defensively if the main theme of their conduct is protection of Soviet power in eastern Europe? After Hitler conquered Poland, the Russians blamed Anglo-French imperialism for continuing the war to overthrow the new status quo. Does fairness mean accepting the less bloody and brutal but still force-based Soviet dominion over Hungary and Czechoslovakia?

When did or will Soviet control of such areas become legitimate, as the Soviet conquest of Georgia apparently has? Welch feels that the dominant Soviet drive is fear, and this makes truculence almost pardonable. But the fear is largely of loss of control of non-Russian peoples, and it is at least as much of internal discontent as of external attack.

Welch gives much space to sorting out prejudices but again raises unanswerable questions. Does impartiality require equal time, so to speak, as though the Surgeon General's report is to be balanced with the statement of the Tobacco Institute? Can American commentary fail to be tinted by strongly colored Soviet commentary? Can judgment of Soviet policy be separated from passions aroused by American policy, especially war in Indochina? Welch points out that American sovietologists of east European background much more often take a hard line than the native American sovietologists do, but he can only point to the problem of deciding whether the former are realistic or embittered, the latter detached or naively inclined to project American attitudes. All, as he mentions, may be influenced by ethnocentrism; all, as he does not mention, may be under pressures of wishful thinking, since it is not easy to envision a livable future unless the Russians are fairly reasonable. To decide who is biassed, one must know the truth.

There should be an academic motto, "Be fair, hide your prejudices." Welch argues skillfully both sides of many questions, but his own slants show through. The study of "Ultra Hard," "Hard," and "Mixed" schools, with no "Softs," is set up to show American writing as unbalanced; this requires placing Fleming and Schuman in the "Mixed" category, although Welch concedes that they are tenderer toward Soviet than American actions. He denies any significant change of attitude during the period of the Vietnamese war, although such change was already apparent when he was writing (1969), and he was flailing a sick horse in the "Ultra Hard" image. Inexplicably, there is no mention of perhaps the most striking act of Soviet foreign policy of the postwar period, the invasion of Czechoslovakia, although there is a one-line reference to the removal of Dubcek in 1969. Soviet postwar expansion is belittled (p. 190) as representing only a small percentage increase of the immense but mostly almost unpopulated Soviet territory. Some would consider it naive to cite the Soviet constitution as indicative of Soviet intentions, to regard (p. 189) lack of reference to expansion of Soviet power in communist statements as serious evidence of lack of such intentions, or to object (p. 73) to the "procedure of treating present practice as indicative of desires for the future," while looking preferably to official declarations. Welch criticizes Kennan for treating Stalin's character as an obstacle to collective security arrangements in the 1930's; is it unimportant that a prospective alliance partner is a murderous tyrant? Basically, he neglects the fact that Soviet foreign policy is subject to no public criticism and to almost no check of public opinion, not to speak of constitutional pluralism.

The methodological remedies offered are not promising. Elaborate treatments of one kind of evidence usually imply excluding other kinds, and figures can lie as well as words. It seems rather futile to derive proportions between "militant" and "nonmilitant" Soviet actions unless they can be meaningfully compared with those of other states, an exercise not attempted. The fifty-page analysis of Soviet treaty-keeping provides a less than reliable guide for policy. The finding is that "the USSR fails to honor a political promise, on the average, in only about one month out of every 120 (or 10-year period) over which it is bound, observing the promise the other 119 months" (p. 228). One who drinks only once in ten years is a teetotaler, but one who assassinates only once in ten years is an assassin. The statesman who has to estimate the worth of a Soviet (or other) promise would do better to consider the political tradition, personalities, political morality, and incentives or compulsions for faithfulness to the engagement.

American writing on Soviet foreign policy could be much sounder, but it may be defended. It stands far above the few works in French and German. If it is "intellectually deficient," as Welch asserts, comparable Soviet writing is not only scanty but frankly devoid of "bourgeois objectivity." Nor is it certain that avoidance of exceptionable positions is an absolute good. My own work on Soviet foreign policy was chided in this journal for "cautious surrender to complexity," and something is to be said for writing from a definite viewpoint. A commitment to objectivity in a subject as tangled as Soviet foreign policy must mean mostly a commitment to openmindedness and to an open and pluralistic order, in which ideas can be measured against contrary ideas. Paradoxically, to stand for impartiality requires standing for the open society, which requires partiality as between Soviet and American values.

ROBERT G. WESSON

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Politics of Southern Equality: Law and Social

Change in a Mississippi County. By Frederick M. Wirt, Foreword by Gunnar Myrdal. (Chicago: Aldine Publishing Company, 1970. Pp. 335. $10.00.)

Speaking in terms of political party symbols, Professor Jasper Shannon once referred to the American South as "the rump of the jackass." But the rump of a jackass may be the object of either a friendly pat or a hostile kick, and much of the literature purporting to analyze southern politics in recent times has been in the nature of a kick. Such is not the case, however, with Professor Wirt's superb account of the effects of national law and law enforcement on the politics and socioeconomic structure of a county in a deep-southern state. Although the focus of Wirt's attention is on a single Mississippi county, his deft political analysis produces sound generalizations that hold not only for that peculiar geographic entity, the former Confederacy, but for political systems generally. Wirt is successful for two reasons, both of which ought to be—but often are not—essential maxims for the study of politics. First, his research methods are impeccable. He not only consulted the literature of southern politics in depth, but he conducted extensive interviews with local and national actors in the drama of social change. This without more would have produced a competent study, but the book goes beyond the collation and analysis of data. It is an exciting and perceptive narrative, and it is so because the author never loses touch with Plato's central concern, the achievement of a just society.

Professor Wirt trains his political microscope on Panola County, Mississippi, typical in many (but not all) ways of the nonmetropolitan South. With its center located thirty miles south of Memphis, Panola is physically and emotionally in the Delta mold. Of its population of 30,000, two-thirds earned less than $3,000 in 1960. One in sixteen inhabitants has no education whatsoever, and less than one in seventy completes college. While very few people of any color are affluent, Negroes live in a condition of "near serfdom." In 1960 only one Negro in the county could vote, and he was a minister who had been registered in 1892. The equality of man in Panola, says Wirt, "can be found only among the wretched." The findings suggest three questions to the author: Should the existing inequality of conditions be altered? If so, is law an appropriate instrument for that end? And how can law be used to bring about the desired results? The first question involves the nature of justice, and with the passage of the Civil Rights acts by Congress in the 1960's, a public moral judgment was made in favor of a redistribution of the conditions of equality between whites and blacks. The author's research efforts yield an emphatic yes to the second question—the appropriateness of law in reducing inequality; and they partially answer the third, since some laws obviously have been effective.

In the past, a serious impediment to law as a means of guaranteeing civil rights has been the adversary system. It did not work for the black man because its two major requirements—a rough equality of parties in their resources and an impartial judge—were totally lacking. As Wirt reasons, "when one adversary far outweighs his opponent in power and status, or when the judge is intimately tied in with the interests of one party, the parity and impartiality assumptions are false, and the model's ideal of justice is frustrated" (p. 109). But after 1960 when a newly organized Civil Rights Division in the Department of Justice began to litigate civil rights in earnest, the parity assumption became more real. During the past decade law has been used as an instrument for change with varying degrees of success in three areas—voting, school desegregation, and economic rights.

Most important in terms of immediate results was voting. By 1967 the lone Negro registrant had been joined by 3500 of his fellow blacks, which meant that more than half the Negro population was now able to vote in Panola. Their votes were being sought by white candidates, and material rewards were quickly discernible—paved or graveled roads to Negro homes for the first time in memory, more and better news coverage of the Negro community in the local press, and the curbing of official violence. Without national laws and without the government's clear intent to enforce them, there would have been little change in Panola County.

School desegregation is more complicated than voting registration, but here too, enforced law did change behavior. When the 1964 Civil Rights Act was passed, just over one per cent of the school children in the seventeen southern and border states were in integrated classrooms. By 1970, after the Department of Health, Education and Welfare had been withholding funds under Title VI, the integration figure reached 40 per cent. As Wirt points out, however, Panola County cannot treat black students equally with whites without lowering the quality of education generally. Although Mississippi ranks high in the percentage of its budget spent on education, a high percentage of a small amount still yields a small amount. Spending more money on education is the only way to equalize opportunities for blacks without impairing quality. But unfortunately those at the top who believe in equality in the abstract back off when the costs are high. Or as Wirt suggests, "as the level of cost to whites increases the effectiveness of law in expanding equality decreases" (p. 215).

In the area of economic rights the law has had the slightest impact, primarily because the target is so diffuse. Employment opportunities in Panola are severely limited, and federal law does little to reach black employment needs. Instead it uses job substitutes—pensions, welfare, food stamps—which deal only with the symptoms of poverty. Even though the author admits that at this point in time, the whites' perceptions of blacks has changed little if at all, the behavior patterns have changed, and it seems likely that the "law will have played a signal role in reshaping perceptions after it has reshaped behavior" (p. 236). But the lesson is clear, says Wirt, that in civil rights matters, southern whites move very slowly toward equality without "direct federal pressures which threaten specific injurious sanctions" (p. 290).

By using all available tools of political analysis, old and new, and by approaching his subject with humane uncertainty, Professor Wirt has constructed a model political essay, an essay that not only informs, but one that points the way to political truth. "Civilization," he suggests, "can be judged by the kinds of moral questions in which the whole population becomes engaged and by the answers they give such questions" (p. 18). Perhaps it is too much to expect that the entire population of the United States ask the right questions, but large numbers of people are asking them, and on occasion, the system gives the right answers.

ROBERT J. STEAMER

Lake Forest College

The Political Economy of Modern Spain: Policy-Making in an Authoritarian System. By Charles W. Anderson. (Madison: The University of Wisconsin Press, 1970. Pp. 282. $12.50.)

For thirty-two years Francisco Franco has governed Spain, surpassing his closest rival (Porfirio Díaz of Mexico) to gain the dubious distinction of the longest continuous one-man dictatorship in the modern world. Until recently political scientists failed to use this regime as a case study of autocratic government, content apparently to describe policy-making as the arbitrary decision of Franco, implemented through an ideologically totalitarian single-party state relying on force. Professor Juan Linz of Yale has begun to revise this hypothesis, and in the process has contributed to the study of comparative government the concept of an authoritarian state (as distinguished from Nazi Germany's totalitarian system). The authoritarian state is characterized by political pluralism which is, however, limited, not responsible, and includes only those groups upon whose agreement the ultimate success of any given policy would depend.

Professor Anderson, working within Linz's framework, has achieved a splendid study of the functioning of political pluralism in the Franco Regime. It will meet the needs of several groups of scholars: Historians of contemporary Spain will find new data and deep insight; Anderson's analytic method, by its very neutrality, enables him to handle material electrified with political passion. Second, for political scientists and economists he creates a working hypothesis about the way authoritarian political systems in similar traditional societies in the postwar world seek to reform their underdeveloped economies. Such is the case in Latin America, where dictators claim to perpetuate old institutions while quietly transforming their function; it is also the case in certain European Socialist states such as Poland, where despite rhetoric to the contrary, the regime has built on old institutions and societal forces (the reputed link between Pax, the prewar Catholic Falange of Poland, with the secret police of the Gomulka regime is a case in point). Finally, this study broadens our understanding of the parameters of viable public participation in the making of contemporary complex economic policies, be it in a democracy or in an authoritarian regime.

The author's great contribution to contemporary Spanish history is a lucid description of the way the political process actually functions. The study is limited to the period 1956–1967, and focuses exclusively on the decision of the Franco regime to abandon its policy of economic nationalism in favor of "neo-Liberalism" (that is, the formulation of the Stabilization Program of 1959, and the two Plans for Economic and Social Development in the 1960's). It encompasses the entire Franco era, however, by describing how the changes developed from existing institutions and policies. By ordering and conceptualizing these empirical data, the author clarifies the way this change emerges from the Regime's *dialogue* with key interest groups: "dialogue" is purposely used to indicate consultation, excluding any critical debate that might bring into question the basic assumptions of the system itself and endanger thereby the foundations of state policy. Yet Anderson's description of this controlled participatory decision making reveals in dramatic fashion the "revolution by stealth" that did in fact occur: Syndicates, the government controlled organizations that served as spokesmen for labor and small enterprises, were excluded from the making of economic policy as was the Falange, the single party. Economic policy was turned over to newly created agencies increasingly administered by technocrats associated with a secular Catholic institute, Opus Dei.

Professor Anderson's contribution to methodology is a refinement and expansion of categories borrowed from the Dutch economists Jan Tinbergen and E. S. Kirschen. His analysis of the political process separates "procedural instrumentation" from "policy instrumentation": The first he defines as "the selection and arrangement of institutions and processes to define public purposes and the means to their achievement," and the second as "the selection, from the equipment of the state, of means appropriate to the achievement of public purposes" (p. 206). His conclusions are intriguing. In this era of managed economies, he finds little empirical data to differentiate the process of economic policy making in Nationalist Spain from that of Europe's liberal democracies; he speculates that this does not mean that the Franco Regime is less authoritarian than believed, but rather that European liberal democracies are more authoritarian than they are willing to acknowledge. His second conclusion is based on an analysis of the alternatives open to Spanish policy makers, and the way in which they have exploited these alternatives. He concludes that the Franco Regime has been considerably more flexible, if not less authoritarian, than conventionally assumed.

The book also assays but does not quite manage to decipher the role of technocrats in policy making, possibly because the Spanish case is too nearly unique to provide a hypothesis valid elsewhere in Western Europe. The technocrat, a phenomenon of the postwar era, theoretically identifies himself as simply a technical administrator who draws upon his expertise to prepare and implement rationally the socioeconomic policies decided upon by the government which employs him. This role contrasts with that of politicians and labor officials —at worst corrupt, at best advocates for only one sector of the economy. Yet given the intricacy of modern economies, such technical administrative functions also become major policy-making factors. Moreover, Professor Anderson clearly emphasizes the politicization of Spanish technocrats but does not explore the consequences, for example, of the membership of senior government economists in Opus Dei; other members of this religious institute who direct major banks, industrial and foreign trading enterprises, and communications media have undeniably brought pressure to bear upon their colleagues, Opus Dei technocrats. This might possibly explain the emphasis of those technocrats upon the industrial sector at the expense of the macro-economy. It might also illuminate that universal function of technocrats which Anthony Sampson describes as "a useful decoy for the prevailing political system." Moreover the author oversimplifies when he categorizes technocrats dedicated to "structural change" as the reform wing of the Falange. Many reform technocrats are strictly that—apolitical experts working to reform and expand the economy, albeit with the undeniable expectation that this in turn will bring major sociocultural and eventually political change.

In sum, Professor Anderson offers a comprehensive theory on the political process in Spain today, one that will facilitate the integration of new data. And this fuller understanding of the Franco regime will in turn enrich the study of comparative government.

JOAN C. ULLMAN

University of Washington

La Presenza Sociale del PCI è della DC. By Luciano Brunelli, Umberto Canullo, Gianluigi Degli Esposti, Giorgio Galli, Anna Lena, Agopik Manoukian, Luciana Pepa, Antonio Picchi, Alfonso Prandi, Alberto Mario Rossi, Bruno Scatassa, Ada Sivini Cavazzani, Luigi Turco. (Bologna: Il Mulino, 1968. Pp. 698. L. 12,000.)

A bewildering array of parapolitical organizations confronts the student of Italian politics. The two major mass political parties, the Christian Democrats (DC) and the Communists (PCI), are tied to groups mobilizing political support in virtually every sector of society. The associations range in significance from the major trade unions and economic cooperatives to small sports, women's, and specialized veterans' groups. Many of these organizations not only are national in scope, but also, especially in certain regions of Italy, maintain headquarters at the grass roots. Without an understanding of the structure, strength, and political significance of these groups, a grasp of Italian politics at either the national or local level remains incomplete.

Because it facilitates such understanding, *La Presenza Sociale* is an indispensable book for any serious student of Italian politics. It constitutes the fourth volume of the landmark series on political participation in Italy sponsored by the Carlo Cattaneo Institute. Fortunately a summary of the whole massive research effort is now available in English in *Patterns of Political Participation in Italy* by Giorgio Galli and Alfonso Prandi (New Haven: Yale University Press, 1970).

The goals of the team of authors who collaborate in the research for *La Presenza Sociale* are first to describe the origins, leadership, membership and programs of the various associations and then to trace historically, from 1946 until 1963, the relationship between an organization and the party with which it is allied.

The descriptive materials are the best ever assembled. In fact, to my knowledge, the volume represents for many of the organizations treated the first attempt to collect data on membership over time. A major defect of these data, however, as the authors candidly acknowledge, is the fact that they were largely furnished by the associations themselves. Distortions favorable to the reporting groups are to be expected and are evident when different groups estimate a single population, e.g., the percentage of unionization among the total labor force. Even so, the compendium of statistics offers guidelines to broad trends and approximate indicators of membership rosters.

The regional breakdowns of these figures are of special interest because they allow us to begin to examine quantitatively the density of the party organizational networks in areas such as the central and northeastern provinces. In these provinces socialist and Catholic traditions have nurtured rather self-contained subcultures, and political loyalties to the Communist and Christian Democratic parties are integral aspects of local values.

The analysis of the relationship between party and parapolitical associations in the 1946–1963 era accents the primacy of Church over party in the Catholic world and the preeminence of the Communist party over the flanking organizations within its cultural orbit. Christian Democracy has been subject to the discipline of the Catholic hierarchy and dependent upon the help of what turn out to be more Church affiliated than party affiliated organizations. From a national perspective these groups affect DC party policy by allying themselves with factions within the party.

While the DC obtains important support from allied social groups, the authors argue that the network of associations maintained by the Communists increasingly offer only marginal help in mobilizing electoral support. In fact, it is asserted that PCI activists are often diverted into parapolitical organizations at considerable cost to party programs.

It is the economic organizations such as the CGIL union, and the network of cooperatives linked to the PCI, that furnish crucial assistance in the form of patronage positions for party loyalists, sizeable if still undefined financial contributions, and large numbers of willing campaign workers. The authors blame the thorough politicization of trade unions for inhibiting the growth of the labor movement in Italy at a time when rapid industrialization and expanding employment favored an expansion of union memberships.

Two of the most significant political changes that have taken place in Italy since 1963—the terminal year of the present study—are the growing movement for autonomy and unity in the trade union movement and the tendency for the Church to disengage itself from intervention in Christian Democratic party politics. These pressures for autonomy and unity will necessarily affect the operation of the major mass parties and, at the community level, probably change the previously isolated subcultural configurations from which they have drawn such dependable support. *La Presenza Sociale* provides the perspective from which to examine these developments.

Although the introduction by Agopik Manoukian is helpful, especially his discussion of political mobilization, one misses a more explicit analytic framework and some synthetic theoretical discussion. Perhaps that is the next step for the team of authors who have provided us with an essential substantive foundation for future analysis and comparison. This volume and the series of which it is a part are very important accomplishments of an increasingly empirically grounded social science in Italy.

ALAN J. STERN
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Political Groups in Chile: The Dialogue between Order and Change. By Ben G. Burnett (Austin: University of Texas Press, 1970. Pp. 318. $8.50.)

This is a bad book, and I will use no intellectual euphemism or academic doubletalk to say so.

There are three major areas of weakness in Prof. Burnett's work: His general knowledge of Chilean history tends to be superficial, his interpretation of what has been going on in Chile during the last decade is highly misguided, and his approach is unsystematic.

His book accepts without a single question the traditional historical misconceptions that American scholarship on Chile has produced. It also accepts, without challenge or doubt, the political corollary of those misconceptions—liberal democracy has strong roots in Chile.

The political development of the country is viewed as leading toward a form of government that happens to match the ideological commitments of the analyst. The bankruptcy of this kind of cultural bias is presently being challenged by scholars in Chile and in this country. The book's failure to take contrary interpretations into account—even if only to discard them—is its most serious intellectual flaw.

Related to the historical interpretation he has chosen to follow is Burnett's analysis of Christian Democracy. He views this political movement with ideological favor and historical inaccuracy. He exaggerates the importance of Christian Democracy and totally ignores the relationship between its triumph of seven years ago and the charismatic appeal of Eduardo Frei—a relationship reflected by the continuously decreasing success of the party at the polls. Mr. Burnett pictures Chilean Christian Democracy as a much more powerful force than it really is and pays insufficient attention to the clash of more permanent antagonistic forces that is now taking place in Chile. In other words, his analysis can hardly help anyone who is trying to understand what is going on in Chile today.

Since the author takes for granted that democratic forms are firmly implanted in Chile, he is able to dismiss the political power of the military in that country with unwarranted ease. These two serious mistakes color the rest of the book. Anyone trying to make a case for the firmness of democratic institutions in Chile would have to provide a very careful appraisal of its political institutions and behavioral trends. The phenomenon of caudillistic leaders dominating electoral politics, as well as the inherent weakness of political parties in that country, must be carefully explained. The constitutional background of the country, which provides for executive predominance, would also have to be carefully interpreted and explained. Burnett's inability to understand the political power of the military in Chile deprives the book of any potential value for those interested in comprehending the political realities of that country. The brake and check on the present popular front government in Chile is the military. One does not have to be an especially keen observer of reality to know this; the reading of Chilean newspapers should make this point quite clear.

In some instances Burnett engages in the kind of analysis that is no longer excusable. He states that "the constitution is regarded as the supreme law of the land and not just a temporary expedient to underwrite a momentary regime" (p. 50). This kind of constitutional interpretation that offers neither psychological theory nor behavioral information as corroboration is of no use to anybody.

Although the book has a 1970 publication date and postcripts that refer to the elections of September 4, 1970, the author has taken no note of the works on Chile published in 1969. This is another serious shortcoming.

Very unfortunately, Burnett's book typifies to a high degree the overall production of Latin Americanists in this country. Even those who use more sophisticated tools of analysis usually end up, like Mr. Burnett, engaged in the indirect propagation of their own political ideas. One can only hope that a new generation of scholars will break with the past and make most of the existing work on Latin American politics as obsolete as it ought to be

FRANCISCO JOSÉ MORENO
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Politics in Sierra Leone 1947–1967. By John R. Cartwright. (Toronto: University of Toronto Press, 1970. Pp. vii, 296. $15.00.)

In many ways, this book departs somewhat from the implicit paradigm that political analysts have previously used in studying African political systems. In the past, they have usually focused broadly on political modernization and political change to delineate the critical variables influencing change and instability in post-colonial Africa. Professor Cartwright's careful, informative study represents a modification of the existing analytical focus. He does not assume that instability is necessarily the dominant feature of contemporary African states; instead, he focuses more narrowly on politics, parties, pluralism, and elite attitudes as the critical variables supporting the relatively stable, competitive party politics of Sierra Leone. In this respect, his analytical framework, mode of analysis, and conclusions can be usefully compared with Kilson's study of political change in Sierra Leone.

Cartwright begins his study with a definition of the social context—the number and location of the Sierra Leone's major ethnic groups, and the key role of the liberated or returned African slaves (Creoles) in the coastal, colony region in early Sierra Leone history. Though Cartwright fails to make the point explicitly, his data suggest that as the colonial regime became more institutionalized, it became more exclusionary and racist in its relations with the Creoles, whose early pre-eminence in the administrative and commercial sectors declined considerably. Once established, the colonial administration initiated the policies stimulating substantial changes, which had differential effects upon regions and groups, in communications, agriculture, and transportation. Since the traditional authorities remained relatively strong, and since there were few educated Africans in the Protectorate, greater representation for the protectorate population was not granted until the mid-'forties, and then largely at the initiative of the British.

Cartwright also sketches the emergence during the 1950's of rural discontent, which produced important alterations in the traditional values and class consciousness of both workers and peasants. In particular, the leaders of the Sierra Leone People's Party, (SLPP), the dominant political party during the tutelary period of self-government, indicated that they were unwilling to respond to mass demands during the riotous upheavals. The rioters objected especially to the abuses of the chiefs, who were increasingly unable to sustain widespread rural support for the SLPP. Furthermore, SLPP's attitude toward occupational groups outside of the traditional framework was extremely rigid: the loosely structured SLPP drew heavily upon the traditional ruling families in recruiting its leadership and consequently made little effort to organize a mass-oriented political party. Finally some SLPP incumbents campaigned during legislative elections on the basis of personal rather than party resources, skills, and support. Indeed, Cartwright contends that election to parliament could occur without the support of the SLPP because the legislative candidates usually had extensive kinship ties to prominent local families and because the electoral districts were single-member constituencies.

Such organizational weaknesses, plus the inability of the SLPP leadership to respond to rural and urban demands, Cartwright shows, gave the principal opposition parties opportunities (largely unfulfilled) to build political support and fostered the formation in September, 1960, of the principal opposition party, the All Peoples Congress (APC). Led by younger men, less closely linked to chiefs, and more representative of northern ethnic groups, the APC energetically—sometimes violently—pressed its demands; but, on the whole, it acted as a responsible opposition party. Despite the opposition, however, the SLPP was able to consolidate its governing position when Sierra Leone became independent in April, 1961.

One of the most interesting chapters in the book analyzes the 1962 election patterns for Parliament Ministers and SLPP backbenchers by party, district, and occupation of themselves and their fathers. Cartwright argues that the 1962 elections produced a critical realignment in Sierra Leone's political structure: a north versus south cleavage replaced the old urban colony versus rural protectorate cleavage; an ethnic cleavage between Mende (Southerners) versus the Temne and Limba (Northerners) crystallized; and a class cleavage between the traditional modern elite of the SLPP and the lower status political strata directing the APC, also emerged. Nevertheless, the moderation of the APC opposition party and the tolerant and adept leadership of the SLPP party leader, Sir Milton Margai, contributed greatly to the competitive, pluralistic politics which Cartwright views as the distinctive attribute of Sierra Leone's political system.

In sum, this is a good although incomplete political history of the many factors shaping recent political developments in Sierra Leone. One major weakness of the book is its avoidance of analytical issues and its failure to place its findings in the context of recent political analyses of Sierra Leone. In addition, Cartwright's analysis of the attitudes of the political elite, party competition, and institutional procedures does not inform the reader about the larger social and political forces simultaneously at work in Sierra Leone. Indeed, his treatment overlooks several crucial issues: Which is more critical for political stability: that there be formal competition between parties, or that there be competition for nomination to offices within the parties; that there be multiparty competition and choice, or that the internal procedures within the political parties be firmly upheld; that national elites hold certain key attitudes and values, or that similar values also exist within the populace; that the analysis focus on leadership attitudes and party competition, or that it also focus more directly upon the interaction of key social groups within the society and within key political institutions? Professor Cartwright fails to deal directly with these questions in terms of political stability. Unfortunately, in the politics of Sierra Leone the parties may win elections, but the political elites, as is increasingly evident, have yet to create a stable (predictable) political system. Consequently, Cartwright's somewhat forced conclusion that the election of the APC, as the governing party, in 1967 marked a critical test for Sierra Leone's competitive party system is not an adequate formulation. It does not help us to understand why the party system ultimately failed or provide insight into the sources of political instability, which have appeared in 1967, 1968, and 1971.

Clement Cottingham

Swarthmore College

Mao Papers: Anthology and Bibliography.

Edited by Jerome Ch'en. (London: Oxford University Press, 1970. Pp. 221, $10.00.)

Jerome Ch'en's 1965 biography of Mao Tse-tung (*Mao and the Chinese Revolution*) has been widely hailed by critics as "the best book about Mao which has been written" and "the best single narrative account . . . of the rise to power of the Chinese Communist Party." Such praise is not to be taken lightly; and it is thus distressing that Ch'en's latest volume, *Mao Papers,* is not worthy of its distinguished editor.

Despite its hard cover and ten-dollar purchase price, this is not really a book at all. It is a disjointed collection of "little known writings" and "instructions" of Mao Tse-tung, sandwiched between an original essay on Mao's literary style and a comprehensive bibliography of the Chairman's published works. The three parts of the book are almost wholly unrelated, and they would have been better published separately, if at all.

What begins in the Foreword as a promising if rather esoteric commentary on the evolution of Mao's writing style, quickly degenerates into an exercise in linguistic overkill, bordering on triviality. Thus, Ch'en treats us to several pages of rigorous textual analysis and comparison of old and new editions of Mao's works, demonstrating convincingly that in the older editions punctuation marks such as commas and full stops appear within quotes, while in newer editions they appear outside quotes (p. xxiv). To take another example, Ch'en finds that "Mao seldom uses '*ti*' to transform an adjective into an adverb," and that "'*hen*' [very] is seldom used; he prefers such expressions as '*fei-ch'ang*' [unusually]" (p. xxiii). These may or may not be significant discoveries (probably they are not), but nowhere does Ch'en posit any criteria of significance.

Ch'en is undoubtedly correct in arguing that a great deal can be learned through comparing Mao's writings "in their original undoctored forms" with subsequent emended versions as these have appeared in various editions of the official *Selected Works.* But his own comparisons (pp. xxii–xxvi) are largely syntactic and stylistic rather than substantive, and their relevance is not demonstrated. Ch'en is also correct in asserting that the marked stylistic inconsistencies in various writings conventionally attributed to Mao give rise to the suspicion that some of these may have been ghost written (pp. xxvi–xxviii). Yet he merely whets our appetites with a few random examples of his own personal suspicions; nowhere does he interpret the significance of his findings.

Turning to the substantive core of *Mao Papers*—a 75-page collection of some of the Chairman's less publicized essays, letters, and speeches, and an 85-page assemblage of Mao's "instructions" from the period 1944–1969—one is immediately struck by the lack of an apparent justification for publishing such a literary potpourri. Most of the writings in Part I are readily available elsewhere, in both Japanese and English; and while Ch'en's personal renderings may perhaps be more elegant than other available translations, this in itself does not justify their random inclusion in this volume. I stress the word "random" because the collection is neither comprehensive nor coherent, nor does it possess thematic integrity. No attempt is made to elucidate the criteria by which certain of Mao's lesser known writings are included, while others, equally important, are conspicuously omitted. One must also question the apparent arbitrariness with which Ch'en has organized the selections in Part I into "Letters" (section 1), "Commemorative Writings" (section 2), "Talks and Conversations" (section 3), and "Written Statements" (section 4). For example, from the point of view of thematic integrity, Mao's November 1959 letter to production brigade leaders (section 1) would have been more appropriately included directly after the Chairman's February 1958 statement on the Party's working methods (section 4).

Part II (Mao's "instructions") reflects some editorial confusion over the dates of origin of several of the entries. Thus, Mao's "Comments on the Seven Good Examples of the Manual Work of Chekiang Cadres" (p. 114) is incorrectly dated June 1966 (the correct date of issuance is May 1963). Similarly, the Maoist aphorism "As soon as you grasp class struggle, it works for you" (p. 131) is erroneously dated October 1966 (the slogan first appeared in February 1963). Also questionable is Ch'en's inclusion of the celebrated "Sixteen Point Decision" of August 1966 as one of Mao's "instructions" (pp. 117–127). Mao certainly had a hand in editing and revising this document; but as one of the Chairman's subsequent "instructions" (p. 129) clearly reveals, he was not its principal author. (This same criticism holds true for the "Sixty Points on Working Methods" of 1958 [pp. 57–76], parts of which were authored by Mao's "arch-enemy" Liu Shao-ch'i.) Conspicuously absent from Ch'en's catalogue of Maoist "instructions" is the Chairman's June 1964 directive concerning "Six Criteria for Evaluating the Success of the Socialist Education Movement." Finally, this reviewer would question the wisdom of including in this collection such trivial items as Chairman Mao's two syllable statement of September

1967 calling on the Chinese people to "Think hard!" (p. 145).

Part III is perhaps the most valuable segment of *Mao Papers*. In this section Ch'en provides a detailed bibliography of Mao's known writings, organized chronologically in some 560 individual entries, in both English and Chinese versions. Here, again, however, and despite the obvious meticulousness of the editor's research, several shortcomings are apparent: many entries are identified from secondary sources—brief passages or excerpts appearing in *People's Daily* (or other publications)—rather than from original texts; in some cases available English translations are not cited; a number of entries cite the *date* of publication, but not the *source* of the text; and many of Mao's shorter essays (including some which appear in translation in Part II) are not cited at all in Part III.

On the whole, *Mao Papers* is a great disappointment. One gets the distinct impression of haste, poor organization, and more important, insignificance. Jerome Ch'en has published in this volume the ephemera—the incidental artifacts—of his life's work, rather than the essential fruits of that work. One might have hoped for more.

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The Mourides of Senegal; The Political and Economic Organization of an Islamic Brotherhood. By Donal B. Cruise O'Brien. (Oxford: Clarendon Press, 1971. Pp. 321. $11.25.)

The Mouride brotherhood has been the subject of numerous studies. Articles, monographs and even, recently, a book (Cheikh Tidjane Sy, *La Confrérie Sénégalaise des Mourides*, Présence Africaine, 1969) have been written on it, to say nothing of lengthy references to it in books on Islam and on politics in Senegal. Mr. O'Brien, however, does in this book what none of the other scholars—Senegalese, French or American—have been able to do. Avoiding the errors in fact and interpretation of the earlier writers, he provides a comprehensive, detailed description of the origins and organization of the group. He presents a fascinating account of the brotherhood based on careful research on French colonial records, more recent government documents and statistics, Senegalese newspapers and other secondary sources, and lengthy interviews with numerous Mouride leaders and disciples and others (both French and Senegalese) who have had contact with the group. There is certainly some duplication here of information already generally known through earlier studies, but O'Brien provides additional detail, often clearing up obscure incidents or relationships that others merely refer to or ignore. In particular, his chapters on the exotic, hard-working Bay Fall, on the work colonies (the *dara*), on the organizations of the urban Mourides, and on the agricultural practices and land tenure system of the order provide fresh insights into the way the group functions.

The Mouride is an unusual brotherhood. It is tightly organized and is based on the extreme subservience of the disciples to the *marabouts*, who offer salvation in return for obedience and work. Due largely to this organization, and the economic perspicacity of the leadership, the group has an important role in the politics, economics, and social structure of Senegal. Thus, O'Brien's case study is valuable for those who seek to understand any of these aspects of Senegal as well as those who wish to study a Muslim brotherhood with great temporal power.

But the strength of the book—its wealth of accurate detail—is also its weakness. The author states at the outset (p. vii) that "the basic factual elements involved in a socio-historical account of the evolution of the Mouride brotherhood seem . . . to be interesting in themselves . . ."; thus the burden of analyzing or interpreting "the principles behind the facts" is to be left to the reader. Thereafter he proceeds in a lengthy description generally devoid of analysis or interpretation. While he certainly evaluates the meaning of specific facts or events for the organization of the brotherhood itself, O'Brien does not evaluate the meaning of the group for the society in which it is placed. Although references are made to the pre-Mouride social structure, to the influence of Islam on that structure, and to the role of the brotherhood in the politics and economics of Senegal, relatively little attention is given to such matters. The extent to which belonging to this group determines different behavior or attitude patterns for members and nonmembers, the effect of the brotherhood on ethnic group relations, the degree to which the order can or cannot influence economic development or social change—these are only some of the important unanswered questions. A greater attention to the *significance* of the specific facts—perhaps through the use of some kind of conceptual framework, or even through simple comparison with other groups in Senegal and elsewhere—would have given perspective to the wealth of information provided. Nonetheless, in terms of what O'Brien set out to do, he has done an excellent job. This book is a full and accurate description of a group which has been much discussed but also much misunderstood.

LUCY C. BEHRMAN
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Development Problems in Latin America. By the Economic Commission for Latin America. (Austin: University of Texas Press, 1970. Pp. 318. $8.50.)

Development Problems in Latin America is something of a documentary history of the growth and development of the ECLA ideology. Like most documentary histories, the book makes dreadfully dull reading. Apart from the introduction, the book consists of unrevised material that originally appeared as early as 1951 in ECLA documents, in the *Economic Bulletin for Latin America,* and in speeches. Probably the most useful of these is the original statement by Raúl Prebisch, for many years head of the ECLA, on the economic problems confronting Latin America, reprinted as the last chapter in the book.

Under Prebisch's leadership, ECLA became the chief proponent of a particular explanation of Latin American economic underdevelopment. The Prebisch thesis was originally an explanation of the reasons for persistent inflation in Latin America. When Prebisch left ECLA to become head of the United Nations Conference on Trade and Development, he had already extended his thesis to countries of the "periphery"—less developed countries not in the Communist bloc. The ECLA ideology is an elaboration of the original Prebisch thesis into a rather detailed explanation of the causes of economic backwardness in Latin America and a set of policy recommendations for restoring long-term economic growth.

In a region where economics and politics often become fused, the ECLA position is but one side of a lively and sometimes acrimonious fight between the Commission and most Latin chancelleries on the one hand and the International Monetary Fund and the economists in Foggy Bottom on the other. Particularly during the Eisenhower era, the IMF and the Department of State were prone to recommend the bitter medicine of devaluation and economic stabilization to Latin Governments experiencing some combination of inflation, balance of payments deficits, and social unrest. To these same governments, ECLA counseled that the basic cause of the inflation was to be found in poorly functioning production and marketing structures. Thus, an inflation rate as high as 20 to 25 per cent might be tolerated to encourage more productive investment of savings. In addition, ECLA advised that governments carefully plan programs to speed industrialization, particularly through domestic production of items previously imported. Economic integration was advocated as a means of providing larger markets for Latin American entrepreneurs. Latin governments were urged to join with other producer nations to limit the supply of traditional exports in order to achieve relatively high and stable prices for these products on the international market.

ECLA recommendations were better received in Latin capitals than was the counsel of the IMF. But one could never tell if the Commission's politically popular policy recommendations flowed naturally from its ideology or if the ideology was tailored to support the recommendations. It is certain, though, that Prebisch was an astute politician as well as an astute economist. The ECLA ideology provided a cohesive rationale for the Latin worker in his opposition to devaluations and the wage-freeze component of economic stabilization, for the Latin capitalist in his pursuit of subsidies and protection from foreign competition, and for the harried Latin politician looking for an external scapegoat for his country's economic problems.

The logic underlying ECLA policy recommendations can be summed up as follows: Latin American countries primarily sell raw materials to the industrialized countries and use the proceeds to import manufactured goods. Foods such as coffee, bananas, cocoa, and sugar are some of the important traditional exports from Latin America. The level of per capita income in most developed countries is sufficiently high that as per capita income grows, the percentage of income spent on food decreases. Hence the demand for Latin American food exports is stagnant. Nonfood exports such as wool, tin, and copper also face a stable or slightly declining export market as synthetic substitutes are found and industries in the U.S. and Europe more efficiently use imported raw materials. Conclusion: Demand in the developed countries for products traditionally exported from Latin America is decreasing. Furthermore, Prebisch suggests that Latin American agricultural producers are generally insensitive to price swings and, with little control on production in the face of declining demand, there is an oversupply on the international market of most major Latin American exports. Therefore, says Prebisch, there is a long-term decline in the export earnings of the Latin American countries.

On the other side of the fence, developed countries have a ready market in Latin America for their exports. The typical Latin American country must either import consumer goods or import the capital goods and some raw materials to produce those same goods at home. Again, according to Prebisch, industrialists and organized labor in the developed countries work together to maintain price levels, thus forcing Latins to buy in a fixed price market while selling their products in a truly competitive and shrinking market. Furthermore, the wide swings in foreign exchange earnings experienced by most Latin American countries has a strong negative impact on their development planning and public investment programs.

A detailed evaluation of the ECLA position cannot be attempted here. Although there is no uniform pattern of decline in terms of trade for less developed countries, it is certainly true that balance of payments deficits are common, that Latin American exports are not growing substantially, and that the demand for certain traditional exports such as wool is actually declining.

The political attractiveness of the ECLA ideology is not dimmed by its possible technical shortcomings nor by the policy alternatives it eschews. In particular, the ECLA approach shifts the attention of the Latin American politician and policy-maker away from opportunities for vigorous trade promotion, improved export procedures, additional in-country processing of traditional export products, and expansion of nontraditional exports. Ironically, the ECLA doctrine points toward postponement of social development programs until the economic base has been built for an independent economy.

For this political scientist, reading *Development Problems in Latin America* conjures up unorthodox images of an international agency acting like a pressure group. Ideology becomes a political tool to be judged by its utility in serving the interests of the clientele. The substance of ECLA's activity is highly political. Unfortunately, the political content of this book is disguised in a well-reasoned but somewhat dated analysis of economic problems. The book is just what it claims to be: an important effort by the Economic Commission for Latin America to synthesize and describe its own efforts. For the much needed explicit analysis of the politics of economic policy-making in Latin America, we must look elsewhere.

GAYLE AVANT

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Science and Ideology in the Soviet Union. Edited by George Fischer. (New York: Atherton Press, 1967. Pp. 176. $7.95.)

This little book consists of four pieces, dealing with recent development in sociology, philosophy, cybernetics and economics in the Soviet Union. (Obviously, the word *science* is used here in what is sometimes referred to as "continental" meaning, *i.e.* encompassing *all* scholarly disciplines.)

Cybernetics. The only topic in the book which has some relation to natural sciences is cybernetics. It is dealt with by Loren Graham of Columbia University in the shortest but most interesting contribution. Originally, the work of Norbert Wiener and other pioneers of cybernetics was met in the Soviet Union with derision, as a "bourgeois pseudo-science." The Czech-born philosopher, Ernst Kolman, first came to the defense of cybernetics in the Soviet Union in 1954, and since then denunciation has rapidly given place to enthusiasm. In fact, cybernetics has been responsible for the renewal of Soviet faith in the possibility of effective central direction of complex national economy—a hope that was being eroded during the period when rapid growth and diversification of Soviet industry made increasingly impossible its control by Gosplan employees equipped with nothing more sophisticated than desk calculators. Furthermore, cybernetics, with its emphasis on feedback mechanisms, has made it possible to consider decentralization of economic leadership and autonomy of individual enterprises not incompatible with central control.

Outstanding Soviet mathematicians, such as Academician Kholmogorov, were brought by their enthusiasm for cybernetics to envisage the creation of artificial life and intelligence as within its potentialities; but these extreme opinions have been rebuked by Marxist leadership. Nevertheless, cybernetics has become the main basis of renewed optimism of Soviet economic planners.

Economics. Closely related to the article on cybernetics is the review of Soviet economics by Herbert Levine of the University of Pennsylvania. It deals not so much with new economic *thought*, as with new *technology* of economic planning. (Professor E. Lieberman, widely associated in the West with the "new economics" in the Soviet Union, is mentioned only once in Levine's article.) Application of computers in economic planning was introduced by mathematicians, such as Professors Kantorovich and

Fedorenko, as early as in 1939.

In 1962, Glushkov estimated that if traditional methods of economic planning were not supplemented by immensely more powerful automatic devices, by 1980 the whole adult population of the Soviet Union would have to be employed by the Gosplan. Developing the annual plan of a single machine tool plant necessitated, even back in 1962, compiling a document of 17,000 pages!

Two mathematical methods, we are told, acquired importance in computerized economic planning: linear programming and input-output analysis. Linear programming, which Kantorovich has importantly helped to develop, permits optimizing production. If applied to subjects broader than the production of a single type of product, it requires the development of more sophisticated criteria of optimization. Kantorovich suggested for this purpose a system of "Objective Evaluators," closely related to the price criteria used in market economics.

In 1961, the first attempts were made to apply input-output analysis—a method used to insure consistency in economic planning—to the total Soviet economy; an annual production plan for 1962 was successfully prepared on this basis. The method permitted proposing several self-consistent variants of a plan; but when such alternative proposals were outlined in preparation of the next five-year plan (1965–70), the national economic leadership apparently was not satisfied with any one of them.

Levine points out, however, that the disappointment and consequent delay in practical application of new mathematical methods to long-range economic planning in the Soviet Union is just that—a delay—rather than evidence of an intrinsic conflict between science and ideology. The first attempts to apply mathematical methods have revealed the qualitative and quantitative insufficiency of the available information. Computer scientists remain optimistic that when data collection has been improved to remedy the situation, Soviet economic planning will resume its progress toward computerization.

Philosophy. If the essay by Richard T. DeGeorge of the University of Kansas seems less interesting than those on cybernetics and economics, this is not the fault of the author, but of lack of exciting innovation in Soviet philosophy. It seems from DeGeorge's article that Soviet philosophy is still restricted to discussion of social topics in the framework of dialectical materialism. No lively discusion can therefore develop on what the author calls the "first level"—that of the only accepted philosophical system. Stirrings of discussion appear only on "second level" in attempts to analyze the laws of development of socialist society. Marx and his successors have provided an analysis of the development of capitalist society, and predicted that change in the economic "base" will automatically lead to changes in the "superstructure"; dialectic materialism held that new "being" would determine new "consciousness." As a matter of fact, however, the Soviet society, even after passing through the stage of "developing socialism" and entering that of "building communism" did not free itself from its predecessors' weaknesses, such as selfishness and lack of cooperation. As DeGeorge points out, the laws of development of the "superstructure" in socialist society had to be formulated with the word "law" increasingly deriving its meaning not from logical consequences of the new "base" but from empirical analysis of the actual situation, using directives of ideological leadership based on this analysis.

This change of attitude has encouraged the undertaking of empirical studies, and, in DeGeorge's opinion, has strengthened rather than eroded the "first level" theoretical structure. Another way of looking at the situation, however, is to say that as long as the questioning of the assertions of "first level" theory of dialectic materialism is not permitted, developments in Soviet philosophy are bound to remain of no particular significance.

Sociology. The longest article in the book is that by the editor, George Fischer of Columbia University, on sociology. The appearance of "new Soviet sociology" was revealed to the West by the participation of Soviet sociologists in international sociological congresses beginning in the early 1960's. Before that time, sociology in the Soviet Union was treated as a purely deductive subject. After that, experimental studies were encouraged, based on distribution of questionnaires dealing with the attitude of workers and students in relation to society.

A journal, "Social Research," was started in 1966; a first National Congress of Soviet Social Scientists was held in the same year, and a learned Council on Concrete Social Research was established. Divisions of social research were set up in several Institutes of the Soviet Academy of Science; the importance of this research was emphasized in speeches by Brezhnev and Kosygin at the 23rd Party Congress.

The main part of Fischer's article deals with several large-scale studies undertaken in Leningrad, Novosibirsk, and Gorky in recent years. The Leningrad study, based on 2665 questionnaires, dealt with workers' attitudes toward work and leisure. It is interesting that a large majority of answers to the question, "What would you like to do if you had more time?" were "Spend it with my family," followed at some distance by "Get more education."

The Novosibirsk study dealt with job preferences of high school graduates. In all social groups, 70–90 per cent of these graduates put "continued education" as their first wish. As many as 82 per cent of graduates from urban professional backgrounds, and 61 per cent of those from "sub-professional" and working class urban backgrounds, but only 10 per cent of graduates with agricultural backgrounds were able to achieve this aim. In the choice of future profession, physics took first place, followed by the professions of pilot and radio mechanic. Biology appeared much less attractive, while construction and agricultural work were near the bottom, with only sales and clerical careers rated still lower.

These surveys were accompanied by very little analysis and comment. More interesting in this respect was the survey of working-class relation to technology, carried out in Gorky. Its authors took the liberty of applying to Soviet society Marx's concept of alienation of the worker from his work. They concluded that alienation still exists, because of the division of labor and other technological and personal factors. In this study, as in most Soviet sociological writings, confidence is placed on further technological progress which in the context of socialist society will help to overcome alienation in some unspecified way. Fischer's article suggests that recent Soviet sociological texts have made little progress from the dogmatic attitude in the past; and he contrasts this attitude with the much more lively, critical discussion in some recent Polish books.

In his foreword, Fischer emphasizes, as the common conclusion of the four authors, that the widely postulated conflict between science and ideology in the Soviet Union does not reveal itself in the disciplines under discussion. Even in the newly encouraged concrete and empirical research, these disciplines tend to strengthen rather than undermine ideology. In this connection, one can remark that the social sciences in the Soviet Union—for obvious reasons—are at the earliest stage of emancipation from ideological control. Physics has relatively easily resisted such control, even if for a while some of its fundamental concepts, such as Einstein's relativity or Bohr's complementarity, have been frowned upon and barred from textbooks. Biology, on the other hand, has had to survive years of ideological dictatorship and purge, originating in its most exciting modern field—genetics—but spreading from there to other disciplines, such as animal physiology and even ecology. The dark age of Soviet biology ended only with the removal of Khrushchev, although the worst persecutions had ceased earlier, with Stalin's death. During these dark years, a large part of the work of Soviet biologists consisted of noncontroversial descriptive activities, leading to volume after volume of "Soviet Flora" and "Soviet Fauna." Sociology, with its surveys almost devoid of analysis, approaches a somewhat similar stage now; while philosophy remains in an almost complete ideological freeze.

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The Social Sciences and The Comparative Study of Educational Systems. Edited by Joseph Fischer. (Scranton, Pa.: International Textbook Company, 1970. Pp. 533. $10.00.)

The book includes twelve papers commissioned for a meeting of the West Coast branch of the Comparative Education Society. These papers sought "to explore comparative methodologies and theories in the four social sciences (anthropology, sociology, political science, economics) that appear to be useful for the study of educational systems" and "to illustrate the variety of assumptions, beliefs, and hypotheses about the efficacy of education with respect to national and individual development that have never been adequately tested by empirical research" (x, xi). The papers are organized around these four social sciences but only few of them meet the stated goals.

The introductory chapter by Fischer, written after the Comparative Education Society meeting, purports to offer a guide for the comparative study of educational systems. Fischer proposes that comparative educators focus on research which establishes the relationships between school and society employing the concepts and methods of the social scientists. He bases this proposal on the assertion that the work of comparative educators has been of low quality—mostly an unsystematic collection of information about schools and students, without any attempt to relate this information to any larger theory that would place education in an interactive role with other social institutions. His general description of the state of comparative education is quite accurate, and certainly much can be gained from training educators in "one or two disciplines" of the social sciences and in systematic field research. The claim that educators have generally failed to contribute to theory building on school and society is beyond doubt. But many social scientists have dealt with education casually—implicitly or explicitly accepting the notion that there is very little to education beyond national school enrollments, number of schools and teachers, number of subjects offered in the curriculum, literacy rates, proportion of GNP invested in education, and the like. Many of the papers in the book, including those by Thomas, Foster, Benson, and Platt, operate under this assumption and fail to consider the qualitative aspects of education. The quality of the curricula, of the instructional methods, the textbooks, the teachers, the subjects offered, and the general social-psychological conditions prevailing in a given classroom and in a given school provide another important dimension in understanding formal education both as an institution and as a process. For example, the manner in which social issues are dealt with in the classroom, if they are dealt with there, makes a great deal of difference in the political behavior of students. The focus only on gross quantitative measures of education to the neglect of the qualitative is a major problem of previous work in this field; and with one or two exceptions the papers do very little to correct this oversight and provide a balanced and accurate view of education.

The chapter by Fred Frey is one of the refreshing exceptions, and it provides one of the better attempts to relate the formal educational system conceptually and empirically to the national political system. Frey perceives the schools as a subsystem in constant interaction with other political subsystems such as labor and business organizations, voluntary associations, and political parties. Education influences and is influenced by these systems. Frey is at his best when he presents and discusses the ways in which education influences the national political systems—through the political socialization of children and youth, through the recruitment of potential leaders, and through direct influences on formal political actors such as governments and political parties. He provides some empirical data from the literature to support propositions on the various political functions that education performs and draws on his own research in Turkey to show the influential role of formal education in political recruitment. Although his political socialization references do not contain studies conducted since 1965 (for example, the significant works of Jennings, Langton, and Zeigler are not mentioned), he does have some good insights into the role of teachers in the political socialization of their students; he says, for example, that teachers . . . "present a mythical, highly idealized picture of the American political system to students who, when they rub up against reality and learn that the picture was misleading in essential respects, then react through political cynicism" (391–2). Subsequent empirical research supports his claim and the general claim that the school in relation to other institutions has not performed well its political education functions. Unfortunately, Frey is not as clear when he discusses the influences flowing into the formal education system from the other national subsystems. The chapter by Talcott Parsons does a much better job on education (defined "as the process, by which, through special institutional arrangements and formal procedures, individual members of societies are brought to 'know,' 'command,' and/or become 'committed to' important elements of the cultural tradition of the society" [201]). But Parsons does not provide any directions that empirical research should take in clarifying the education-society nexus, and in general, his chapter does not provide as good a framework for sociology as Frey's chapter provides for political science.

The political science section, of which Frey's is the lead chapter, includes papers by Abernathy ("Teachers in Politics: The Southern Nigerian Case") and Guyet ("The Clerk Mentality in Burmese Education"). Abernathy's chapter violates most of the recommendations that both Fischer and Frey suggested for the study of comparative education. The author makes all kinds of claims about teachers and children in Southern Nigeria but offers very little in the way of empirical research. Nor does he relate the subject at hand to any explicit theory of education and society. The chapter by Guyet is more data-based and less impressionistic than Abernathy's, but it overgeneralizes the experiences of an elite school in Rangoon to all Burmese education.

Read selectively, the book can provide some direction for the systematic study of comparative education. But unless the qualitative aspects of education are clearly defined and systematically studied, work in this field will continue to be incomplete.

Byron G. Massialas

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Historical Studies of the English Parliament. Edited by E. B. Fryde and Edward Miller. (Cambridge: Cambridge University Press, 1970. 2 vols. [Vol. 1, Origins to 1399; Pp. 387; Vol. 2, 1399 to 1603; Pp. 350.] $11.00 each vol. Also available in paperback.)

The editors have collected a number of articles by distinguished scholars dealing with the history and development of the English parliament during the medieval and Tudor periods, ranging chronologically from a study by F. W. Maitland, first published in 1893, through contributions by Plunknett, Jolliffe, Neale, and Helen Cam to a study by one of the editors, dated 1969. Each editor contributes a 30-page introduction to one of the volumes, in which he supplies the connecting links in the chronology of parliamentary development. The volumes are dedicated to the memory of Helen Cam, late of Harvard University, whose name, along with that of the Italian Antonio Marongiu, is pre-eminent in the field of the comparative history of parliaments. Of the scholars whom one would expect to find included in a collection of this kind only Pollard is absent. His exclusion may be explained by the editors' mention of articles they were unable to get permission to reprint.

The mother of parliaments has two histories. The difference between them is particularly evident for the period covered by these volumes. One history is predominantly factual. It is more concerned with what parliament did than with the implications of parliamentary activity. The other is a history of the idea of parliament. For this history the inaccurate and wishful thinking, as well as the brilliant theorizing, of a Fortescue, a Coke, and even a Montesquieu may have greater importance than the meticulous scholarship of medieval historians.

The studies reprinted in these volumes are definitely history of the first type. It is the fact of medieval parliament and not the idea of parliament that is elucidated. Some of the authors indeed assume a polemical attitude on this point and seem to be motivated by the desire to show how poorly the facts of history support the enthusiasts who have ascribed their own motives to and inferred their own conclusions from that part of the power struggle in medieval England that was centered in the parleys in which the king, his councillors, and the more articulate of his subjects participated.

The political scientist, like the historian, is concerned with both kinds of history of parliament. Just as the second type is essential to, among other things, the theory of liberal democracy, so the type exemplified by the volumes under review gives valuable insights into power politics and the legislative process.

The older monographs reprinted here are the sources for much of the conventional wisdom found in the better accounts of the history and development of the English parliament—accounts such as Bryce Lyon's recent and highly readable *Constitutional and Legal History of Medieval England*. The insights they revealed may once have been revolutionary but they are now commonplace. By far the most valuable part of the collection is made up of the studies by contemporary scholars. Particularly recommended are Helen Cam's contribution on medieval legislators and medieval representation, T. F. Tout's essay on the English parliament and public opinion, and B. Wilkinson's account of parliament's role in the deposition of Richard II and the accession of Henry IV, all found in the first volume; and that of J. R. Lander on bills of attainder, and those of G. R. Elton and J. E. Neale on aspects of the Tudor parliaments, found in the second volume. The political scientist who reads these more recent monographs is almost certain to acquire in the process some new and significant ideas on the function and functioning of parliaments.

John Clarke Adams

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Colonial Issues in British Politics 1945–1961. By David Goldsworthy. (Oxford University Press, 1971. Pp. xiii, 425. $14.50.)

This is a good and useful book within the framework in which it is written. Critical comments about it are really a criticism of the framework—criticism not as an argument for relevance or for class-conscious analysis but simply on the grounds that telling the story within a narrow framework leads to an account which is not complete even in its own terms. The last decades of the British Empire certainly pose a large-scale problem, and large-scale problems have to be split up into smaller problems if they are to be handled in a properly scholarly way. Mr. Goldsworthy has subdivided them a little more than will be realized by any but the most cautious readers of the title of his book: as this is a study of party political attitudes to affairs handled by the Colonial Office, the two most important events between 1945 and 1961 directly connected with imperial rule are discussed only in passing, because independence for India was a matter for the India Office and the Viceroy, and the Suez operations of 1956 were a matter for the Foreign Office.

Events of this importance are not easy to leave out of the underlying pattern of development. This book argues that in 1945 and under the Labour government, British colonial policy could be bipartisan because both parties agreed that measures of economic development were desirable and believed that independence for most colonies was almost immeasurably far off; bipartisan agreement broke down in the 1950's as the Labour party moved to a more anticolonialist position and eventually committed itself to independence on a basis of majority rule for all colonies; bi-partisan agreement was then restored in the early 1960's when the Conservative government came to accept the same policy and put it into effect. Because of his emphasis on Colonial Office affairs, Mr. Goldsworthy does not point out that the debate on India in the 1920's and 1930's had established the Labour party as more anti-imperialist than the Conservatives (and it is a bit odd to say that from 1929 to 1931 the Labour government "adhered to the report of the Simon Commission which had recommended no change in Indian representation" [p. 116], and to make no reference to the Labour government's approval of Halifax's declaration on Dominion Status); nor does he mention the way that Suez reinforced the anticolonial image and the anticolonial feelings of the Labour party.

Mr. Goldsworthy is well aware of the difficulty of establishing definite conclusions in the restricted area he has chosen. There is a great deal of useful information about the Labour and Conservative pressure groups—Fabian Research Bureau, Africa Bureau, Movement for Colonial Freedom, the "Suez group," the Joint East and Central Africa Board, and so on—which tried to move the leadership of their parties into more clear-cut policies. There is also an interesting discussion of the way pressure groups apply pressure to the government within the framework of party politics: at the factual level there are useful notes on parliamentary questions, adjournment motions, and other aspects of political activity, and at a more theoretical level there is a model, illustrated with examples from the late 1950's, of the way the Opposition may influence the policies of third groups negotiating with the government,

Mr. Goldsworthy clearly wants to find examples which show that the activity of the Labour opposition had a decisive effect on the course of events, and in a very sensible analysis he shows that the Opposition did indeed have an effect in the restoration of Seretse Khama and, more generally, in setting the scene for "the wind of change." But, at the risk of going off into the realms of speculation, the more important effect of the Opposition is surely to be seen in the limits it placed on what the government could think of doing. A comparison with France may make things clearer: The SFIO did not take an anticolonial attitude and after 1956 was as committed to the war in Algeria as any of the parliamentary parties to the right of it. As a result there was no real prospect that an expensive and unsatisfactory colonial war could be turned to the disadvantage of the government. In England the Conservatives could be fairly sure (especially after Suez) that the Labour party would oppose any colonial war that was going badly. This was no problem for the government over Kenya, but it did impose limits on what the government could do in Cyprus; Mr. Goldsworthy notes the effect on EOKA of Labour attitudes, but he simply takes it for granted that no British government was going to behave like the French in Algeria or Indochina.

The inevitable difficulty about writing this sort of book is that governments do not choose to reveal the ways in which they decide on policy. The most easily visible result of this reticence is the frequency of Goldsworthy's references to Lord Chandos' *Memoirs;* they are the best evidence there is, for no other Colonial Secretary has written as lucidly and attractively about his term of office, but they are of course the case for the defense. The Opposition—especially when it is the Labour party—is relatively easy to find out about; the government is always harder to examine. This book certainly makes it clear that party politics had some effect on the policy of decolonization, but does not manage to get that effect pinned down; perhaps it is asking too much to expect that it could.

TREVOR LLOYD

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The Japanese Challenge. By Robert Guillain. Translated from the French by Patrick O'Brian. (Philadelphia and New York: J. B. Lippincott Company, 1970. Pp. 352, references, index. $8.50.)

The Emerging Japanese Superstate, Challenge and Response. By Herman Kahn. (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1970. Pp. xiii, 274, appendix, notes, selected bibliography, index. $7.95.)

Japan is once again beginning to attract general interest, probably because of its having acquired the number three position, as measured in gross national product, among the world's nations. Both of these books address themselves to and attempt to explain this economic miracle. Kahn has a number of projections for Japan's economy in the year 2000: the low is a one-and-a-half trillion dollar (U.S.) GNP, the high a four-and-a-half trillion dollar GNP. Guillain, less sanguine, eschews constructing specific scenarios of possible Japanese "futures."

Why have the Japanese worked so damnably hard? What drives them to be "number one," whether it be in yearly percentage increase of the GNP, or in having the fastest train, or in building the biggest ships, or having the best cameras or TV sets? Our authors struggle with answers, but I am not sure their efforts are as successful as the Japanese own struggle to "get ahead."

In effect, two elements are considered. One is attitudes. Guillain succumbs to the making of grand—and unprovable—generalizations. He notes that their "driving zeal for work . . . is probably instinctive" and that it is a "characteristic of the yellow race in general" (p. 80). Kahn relies—in a chapter appropriately entitled "Some Comments on the Japanese Mind" —on long quotations from the writings of mostly American specialists. His favorite author is Ruth Benedict. In his "Appendix," he provides a synoptic overview, including lists of characteristics and comparisons with other people, e.g., "The Japanese Personality in Comparison with Englishmen." One cannot help wonder whether it is all to be taken seriously, because what becomes most apparent is that the Japanese are extraordinarily contradictory. They do, of course, work hard (and play hard when given the opportunity); of this there can be no doubt.

A second set of factors is structural. Both authors expatiate on the virtues of life-time tenure in corporations, "house" (company) unions, early retirement, paternalism, and substantial fringe benefits. In short, subservience of self to the family or company or nation: Japan, Inc. Somehow the presumed virtue of "individual initiative" which is supposed to fuel "economic growth" is sublimated, and everyone is dedicated to the greater glory of Hitachi, Honda, Matsushita, Sony—and Japan. Guillain is somewhat stronger than Kahn in criticizing the degree to which the smaller companies absorb the shocks of (thus far temporary) recessions and the relatively ruthless manner with which government and industry leaders decide that certain products will no longer be manufactured, resulting in "throw-away industries." American and European businessmen and industrialists would be well advised to read either or both books, particularly if they are faced with Japanese competitors or would like to participate in joint ventures with the Japanese.

Neither Guillain nor Kahn devotes much thought or space to domestic politics. They compensate by discussing Japanese foreign policy, especially its trade and aid components, at some length. Both stress what Guillain terms the "Asia-Pacific Zone" and Kahn computerizes as NOCPA (Non-Communist Pacific Asia). Guillain is far more willing to include trade with China as an important ingredient in Japan's future. Kahn conceivably sees this possibility more through the prism of ideological and political restraints imposed by Peking. In this connection it may be worth mentioning that when I asked one of my Japanese friends what he thought of Kahn's reported comment that the twenty-first would be "Japan's century," he responded with, "If, so, the twenty-second will be China's."

On one issue—that of "the bomb"—our authors differ widely. Guillain's chapter title "Greatness Without the Bomb" summarizes his view. Kahn is ambivalent. He predicts unequivocally that Japan will "start the process of acquiring nuclear weapons" during the 1970's (pp. 165–66), but adds that he hopes he is wrong in his prediction. Kahn is far more sensitive to the fallout—mostly highly adverse—that such a move by the Japanese would engender than certain quotations taken out of context might lead one to believe. Nonetheless, one has the uneasy feeling that Kahn cannot take the challenge posed by the Japanese seriously without their having nuclear weapons. It may boil down to a simple syllogism: The superpowers have nuclear weapons; Japan is a superstate (emergent to be sure); *ergo,* it too will have a nuclear arsenal. This kind of comment, coming from one of the voices of authority, cannot help but strengthen the position of those Japanese who aver that true respect cannot be gained by economic means alone. In effect, Kahn accepts the "Chinese threat" without really asking if that "threat" would be increased or decreased by Japan's having the bomb. He assumes that the Chinese "threat" would be diminished, but such an assumption may well be the height of folly or irresponsibility or both.

It is really in their respective discussions of "the bomb" and related foreign policy issues that other differences between the two books become apparent. It is not only that Guillain is a Frenchman (I prefer his original title, *Ja-*

pon: Le Troisième Grand to the English) and Kahn is an American, though no doubt this is one factor in explaining their divergent approaches. Probably more important is that Guillain, as permanent Far Eastern correspondent for *Le Monde,* is an old Asia hand, which Kahn says explicitly he himself is not. Guillain began his lengthy sojourn in Asia by covering the Sino-Japanese War in 1937–38. He also spent the World War II years as a not necessarily willing guest of the Japanese, and has experienced more than just the sunny side of the Japanese.

Kahn's vision is considerably more restricted. He marvels at the wonder of Japan and her people. His occasional demurrers about possible areas of darkness are similar to his arguments against Japan's acquiring nuclear capabilities; they may be present, but the weight of the evidence is all on the other side. His book, which is really a series of pensées on his interaction with the Japanese, is provocative and challenging, but I felt more comfortable reading Guillain. Both books add to our capacity to consider the old question, "Whither Japan?," but more with regard to her economy and foreign relations than her domestic politics.

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East Central Europe: A Guide to Basic Publications. By Paul L. Horecky. (Chicago & London: The University of Chicago Press, 1969. Pp. 956. $27.50.)

This work is a companion volume to an earlier bibliographic work entitled *Southeastern Europe: A Guide to Basic Publications.* These two works represent the only two full-length annotated English language bibliographies that treat the area usually referred to as "East Europe." Many bibliographies, of course, are organized around special topics or treat the literature of various states of the area. The work was sponsored by the Subcommittee on East Central and Southeast European Studies (of the American Council of Learned Societies and the Social Science Research Council) with the support of the United States Office of Education.

A work of this nature is a tool rather than a platform for the expression of opinion. The sixty-two scholars who compiled this major bibliographical work include some of the most eminent experts in the fields covered. The task confronting them was a monumental one; the past fifty years saw the publication of 600,000 titles pertinent to the area. These were published both within the enumerated states, and about the area by interested authors and scholars. Of these, according to the editors' estimate, about 120,000 titles were potential material for inclusion. The finished work includes 3,380 bibliographical entries, chosen with no evident debilitating biases.

The widely used term "East Europe" has been an historical and geographic misnomer when applied to this complex region, much of which lies in the heart of Central Europe. The definition of East Europe as that area where the Soviet Union exercises its political influence has been fostered by the very structure and organization of Slavic and East European studies in the American university. The work at hand avoids such oversimplifications by its title and organization.

The volume is subdivided into six discrete parts. The first of these treats those bibliographical entries which are concerned with the area as a whole or address themselves to problems of areawide interest. It also contains such specialized topics as "Jews in East Central Europe" and "The Council for Economic Mutual Assistance." The subsequent four parts deal with the states of the area, namely, Czechoslovakia, East Germany, Hungary, and Poland. The sixth and final section deals with such exotica as the Sorbians (Lusatians) and Polabians.

The bibliography for each country is subdivided into eight subheadings, organized along topical lines. These are The Land, The People, History, The State, The Economy, The Society, and Intellectual and Cultural Life. While these categories are not strictly disciplinary, they offer adequate scope to accommodate the interests of those in the social sciences and the humanities. Since much historical research represents valuable social science data, the overwhelming weight of the bibliography lies in the area of social science. The work includes not only books but also vital articles and monographs. It lists the leading journals in each of the cultural and national areas. The entries are not restricted to works in the English language, but include important contributions in the leading languages in which area research is done, namely German, French, and Russian, in addition to the languages indigenous to the area. The English annotations provide rudimentary insights into the nature of the publications, and the editors use the Library of Congress standards for transliteration and annotation. All the entries are numbered; the index refers to entry numbers rather than to page, which is helpful.

The utility of this volume to the political scientist cannot be over-estimated. It represents a first-rate research tool. Most political scientists are well informed about the literature in the immediate area of their research interests and will generally know more than is included here. The greatest utility of this work is as a ready source of bibliographic information in areas where the researcher departs from his most immediate field of specialization. The volume is also a teaching tool, since instructors are confronted by constant student requests for bibliographical information which may be remote from their primary field of interest. One can scarcely imagine an institutional library that could omit this work from its reference shelves.

The richness of the indigenous source materials and the objective treatment they are given set a high standard for future bibliographical work in this field. Political scientists should welcome the appearance of this volume as a landmark in the emergence of "East Central Europe" as an autonomous field of inquiry.

George Klein
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The Structure of Local Government. By Samuel Humes and Eileen Martin. (Mouton, 1969, pp. 675.)

Changing Patterns of Local Government. By A. F. Leemans. (Mouton, 1970. Pp. 224. 16 guilders.)

The unifying theme of these two books is concern with the institutions of local government in countries through the world. Humes and Martin, updating their 1961 book on this subject, treat the structure of general-purpose, largely representative governments by comparing the institutions of which they are composed with respect to role, interrelationships, and public service responsibilities. The authors also describe separately the organization of local government in 81 countries. They are impressed with the great differences among institutions, noting that "he who is familiar with only his own system of government knows none" (p. 24).

A. F. Leemans also deals with the structure of representative local units. The heart of his book is a description of patterns of local governmental administration around the world. Here, however, the focus is on how such institutions have been reformed, and on mechanisms currently in use for improving institutions. Excerpts from reports on recent reforms in Sweden, Toronto, and England are appended. The book also contains examples, but not detailed descriptions, of local government in many countries.

There is an obvious scarcity of material describing, in a sophisticated manner, the specific institutions that operate at the local level outside the United States. If either of these books provided us with a high quality research aid by describing in a definitive way the structure of local institutions, I might be moved to forgive them for their intentional disregard of whether institutions work as the law specifies, and their sparse attention to how the sociocultural environment influences the operation of local institutions.

The first portion of Humes' and Martin's book comes close to being such a tool. But it is flawed by the fact that its recognition of problems at the core of local administration, to say nothing of those implicit in social science research, is so muted as to efface the issues. Humes observes, for example, that technical efficiency and popular participation are apparently conflicting emphases in local government structuring. The debates in this country over the outcome of the poverty program and other "participatory" programs come immediately to mind. In discussing the resolution of this conflict, Humes tells us only that because this conflict is present, two types of people are needed in local government: the politician and the administrator. This is of no more help to the program officer than to the researcher.

For Americans, the interrelation of local and national government is among the most important subjects that can be illuminated by comparative local research outside the U.S. Given the American tradition of local self-government, the study of local politics has been to a great extent isolated from national affairs here. Humes does stress that "there is a vast maze of means by which the central governments ... can control the decisions and actions of local units" (p. 192). He then tells us how difficult it is to describe such central-local relationships because only a tiny proportion of these interesting activities appears as events that can be analyzed. But his review describes the tip of this iceberg more than what is beneath, as he himself admits. The section therefore ends up being less instructive than the institutional discussion by A. M. Walsh in *The Urban Challenge to Government* (New York: Praeger, 1969).

The descriptive sections on 81 countries prepared by Eileen Martin are also disappointing. A spot check suggests that while these sections are essentially accurate, they are difficult to use and may not include some of the important institutional detail researchers would presumably need to begin to understand how local government works. I compared the section on Chile with an excellent recent monograph on Chilean local government, published before Humes' and Martin's book appeared. (The monograph is Peter Cleaves' *Development Processes in Chilean Local Government*, Berkeley: Institute of International Studies, University of California.) In this case it appears that the most basic source material, a treatise on municipal law listed by Martin without date or publisher or apparent source, could have been cited in a more complete fashion. In addition, a reading of Cleaves' monograph suggests that important laws which consolidated the power of the provincial *intendant* in Chile are not mentioned in the description, leaving the reader without an adequate sense of the growing penetration of provincial affairs *de jure* by the central government. One can also pick up similar problems of nuance in other sections. For example, the description of Mexico indicates the sources of the revenue of Mexican municipalities and the control of the states over them in some detail, without mentioning the relative insignificance of state and local revenues outside the Federal District (in 1963 the municipalities spent about $3.30 US per capita), or the fact that the federal government itself, rather than the states alone, matters in municipal finance.

My impression is that we have been given citations to some of the available materials the author used on each country, not a bibliography of outstanding or comprehensive source materials. And we have been given descriptions based on constitutional documents and questionnaires filled out by officials which lay out the general formal structure of local government in many countries, with so little interpretation that one would almost immediately have to consult other sources even for institutional comprehension. An attempt to analyze the actual life histories of issues and public expenditures would have greatly enhanced the volume even on its own terms.

A. F. Leemans' book also starts out with promise but has the limitations of many similar prescriptive "handbooks" meant for field personnel. The book opens with a commendable effort to specify why countries introduce institutional reforms which change local governmental structures. An interesting chart of the mix of such objectives in the reorganization of five local government systems in different time periods is included. This is followed by a listing of the environmental conditions which shape local institutional structure. But one searches in vain for what should follow: the specification of how different institutional changes are related to achieving such objectives and the conditions under which they succeed or fail.

Leemans makes some effort to associate institutions with objectives, but he never relates these to environmental conditions. The existence of efforts by others to link environment and institutions at the local level in numerous countries makes the absence of such analysis here particularly disappointing. Furthermore, the discussion of the relationship of institutions to outcomes is mainly an outline of possible consequences. There is too little effort to establish in detail the complex trade-offs among different kinds of performance. On the level of sheer description, however, the book is more useful than the more expensive Humes and Martin volume.

One of the major determinants of the nature of local government clearly is its structure. But the net effect of focusing attention solely on the principles of institutional arrangement is in both these cases to leave the reader little better equipped to project the likely behavior of or outputs from institutional systems.

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The Postwar Development of the Republic of Vietnam: Policies and Programs. By the Joint Development Group. (New York: Praeger Publishers, 1970. Pp. 584. $19.50.)

There will be two unfair features to this review. First, it was not possible in a thousand words to describe the thousand days of the Joint Development Group's tenure in Saigon or the 552-page volume it produced, and such an attempt will not even be made. The only assessments offered are those I have heard made by Vietnamese themselves. Second, any political scientist reading the report of an essentially economic task force is likely to do it less than justice, and readers interested in an economist's appraisal can find a good one in Albert P. Williams, Jr., "South Vietnam's Development in a Postwar Era: A Commentary on the Thuc-Lilienthal Report," *Asian Survey,* XI (April, 1971). I will suggest, instead, why a political scientist should read or—as is more appropriate with government reports made into books—refer to it.

Of the countless consultative reports the U.S. government has supported on Vietnam, this volume represents one of the most comprehensive, committed, and compelling—if only because it *was* a joint effort (although the products of each side were published separately) in which Vietnamese were treated as equal participants. It was also (perhaps unfortunately) an independent study—one that consciously sought to remain above the intrabureaucratic politics of the GVN and U.S. establishments. In so doing, however, it manifests an aloofness from the war and its politics: "postwar development" is described in a tone which makes one forget that the present conflict is one from which many Vietnamese feel their society and its politics will never recover. Certain anomalies result in an approach which describes a development program for a stable Vietnam that most political analysts, USAID officials, and Vietnamese have never seen. The plight of 5 million refugees whose future promises to be bleak and tragic, for example, receives a scant 13 pages of coverage in appallingly simplistic terms. The case is not clearly made that the infrastructural developments suggested for the next ten years (and for an estimated 10 billion U.S. dollars) will also yield a coherent program of social development for this unfortunate population. To the North Vietnamese, the credibility of the "deep reservoir of compassion and generosity to the victims of a war" (p. v) to which Mr. Lilienthal refers in his Foreword must appear less than convincing when an estimate of the war's damage is made. Put at slightly more than one-quarter billion dollars, the figure includes only damage due to "enemy" (i.e., North Vietnamese-Viet Cong) activities.

The significance of such a report, however, will not depend upon the impact that its tone, scope, and approach have upon a reviewer. The crucial question is how the report has affected Vietnamese. This concern puts in somewhat different perspective the assertion of the preface that "In our opinion, the programs outlined here, if followed in their essential respects, provide assurance of the economic future of Vietnam" (p. vii). If this report has indeed provided such a framework, it will have rendered great service. If it serves as a springboard for Vietnamese reaction and thus allows the Vietnamese to clarify their own program for development, the service rendered will have been equally great. In either case, the volume should be referred to now for the wealth of information and data it contains and read at a later point by those who will write the history of Vietnam "after the war."

When the English version of the report first appeared in Saigon and Washington, it was regarded as a detailed, comprehensive, and humane analysis of possible courses of Vietnam's future development. The Foreword and the Preface to the present volume suggest that it was something more, a hope which, unfortunately, has proved unfounded. Those associated with the project did not write it as a planning document, nor did they advocate its use as such. Instead, they maintained, it was designed to focus attention upon the need to begin "postwar" development even before the war's end. I find that the scholarly criticism the Report has received misses the mark: Professor Hla Myint, for example, in his report to the Asian Development Bank (*Southeast Asia's Economy in the 1970's*, Manila, 1970) contends that:

> The problem is not that which is envisioned by the . . . Report. That approach is characterized by "the making out of a shopping list of desirable programs which Vietnam would like to adopt in some future period of peace—if and when peace arrives and if resources can be found." Rather the problem is what should Vietnam do *now* to remedy the weaknesses and distortions in her war-time economic structure so that she can make more effective use of the external resources available to her *now and during the transition period to peace.*

I doubt that the Joint Development Group would contend otherwise or dispute such a fundamental imperative.

A different sort of criticism has emanated from Vietnam and in it may lie the Report's real significance. My own interviews with 280 Vietnamese government and political leaders during 1969, for example, revealed that more than half had read the study, and a considerably greater number were familiar with certain of its sections. Portions have been serialized in political journals, and it has been a subject of discussion in recent books devoted to the coming political struggle, such as Nguyen manh Con's *Hoa Binh: Nghi Gi? Lam Gi?* [*Peace: What to Think? What to Do?*], and Nguyen Mau's *Vietnam: Chien Tranh va Hoa Binh* [*Vietnam: War and Peace*]. Most of the attention the report has received can be summarized by the following remarks of a Vietnamese scholar-official in an interview:

> Currently, we have no universally accepted planning document. The Lilienthal-Thuc group's report has been severely criticized on both the American and the GVN side for the failure of the group to consult with any of the officials responsible for the planning and execution of the programs suggested. While AID approved of the group's attitude of independence which meant that only a few of us were consulted, we, by and large, did not.
>
> Lack of acceptance of the report by our government at the working level is no doubt caused by some resentment at not being consulted, but also by the fact that much of what is recommended is well beyond our ability to implement. So far an interministerial review committee has been set up to review it, and while this committee has an impressive title it does not contain any significant or impressive members. What postwar planning we do is done without coordination between sections in the ministries.

In 1969, the Vietnamese associated with the Report convened as a new government ministry in a basically hostile environment, while D and R's contract with the U.S. government ended in 1970. Injected into the new ministry, however, was an element lacking in the Joint Development Group—namely, an attention to politics and a recognition of the need for postwar development to stimulate the creation of political goals for the GVN. As one member of the ministry observed of its task in an interview:

> We have the plans for future development—or at least the capacity to develop them—but we lack political direction. Postwar development is a political problem as much as it is an economic one. It will be meaningless unless we have a political strategy for the coming struggle. This is why, for example, we are trying to convert many of our plans into legislation. In this way the GVN will be committed to specific policies by its politicians and not the economists and committed in such a way that a new cabinet or a new Minister of Economy will not be able to change plans or ignore them so easily.

To the members of this new ministry, postwar development has come to represent a process of political committment, requiring a strategic as well as an economic conception of the postwar milieu. The significance of the Report may thus lie in what it failed to appreciate. Although it shows no awareness of the political requisites of development, its publication has helped to create a process by which Vietnamese politics and postwar planning can be linked.

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Modern Korea: The Socialist North, Revolutionary Perspectives in the South, and Unification. By Kim Byong Sik. (New York: International Publishers, 1970. Pp. 319. $8.95, cloth; $2.95, paper.)

This book is an attempt to present a revisionistic political history of Korea and to tell the role played by North Korea's Premier Kim Il Sung in the completion of both the Korean and the worldwide communist revolution. It is an English translation of the book originally written in Japanese in 1969 by another Kim, who is a Deputy of North Korea's Supreme People's Assembly and vice-chairman of the (Communist) General Association of Korean Residents in Japan.

The book is divided into three parts. In the first part, comprising almost two-thirds of the book, the author has dealt with the theory and practice of socialist construction in North Korea; in the second part, he has underlined the method of achieving political unification with South Korea; and in the brief third section, he has tried to show the connection between the Korean Revolution and the World Revolution as a means of communizing the world.

Kim Il Sung, we are told, conceives that his principle of *Juche* (literally, "self-reliance" and used here in the American sense of "Yankee ingenuity"), when applied correctly, can solve all the problems of socialist construction and economic guidance in North Korea—the role of the dictatorship of the proletariat, the peasant market, the system of political and material incentives, the commodity-money relations, the planning and tempo of socialist development, and so on. It can also be applied to strengthen unity among the socialist countries and the communist parties in such a way that the "U.S. imperialists" and their allies can be destroyed in the near future.

What, then, is the idea of *Juche* and how was it to be applied to North Korea? The concept seems to embrace "the principle of self-reliance in ideology and in politics, in the economy and in military affairs." It "rejects subservience and dogmatism," stating that "Marxism-Leninism is not a dogma, but a science." Through *Juche,* the author tells us, Marxism-Leninism was applied to the concrete realities of North Korea (pp. 10–11). He thus claims that during the past four decades, Kim Il Sung led the Korean people in the fight for national liberation, especially during the 15 years of armed struggle against the Japanese (p. 23). (This simply is not true, for the present Kim Il Sung adopted his name only in 1946 after the deceased national hero of the same name.) Kim is also supposed to have brought the people's democratic revolution to North Korea by forming a broad united front and by instituting a series of democratic reforms—such as land reform, nationalization of key industries, rights of labor, and equal rights of the sexes (pp. 23–37). But the author says such "preparatory work for the transition to socialism had to be suspended when U.S. imperialism launched its predatory armed aggression against the North on June 23, 1950" (pp. 38, 231). This flatly contradicts his later statement that "... [in] the Fatherland Liberation War [the Korean War, 1950–53] ..., as a result of the victorious advance of the [North] Korean People's Army, a large part of South Korea was liberated and organizations and people's committees were again organized in the liberated areas, various democratic reforms were carried out, and revolutionary enthusiasm of the masses ran very high" (pp. 245–46).

Since 1958 North Korea has been engaged in a "socialist revolution." Again, we are told, Kim Il Sung has relied upon the *Juche* idea in eradicating "capitalism, . . . individualism and egotism" in order to bring about the complete victory of socialism (pp. 71–74, 113). In dealing with the most fundamental area of Marxism-

Leninism—the interrelationship between productive forces and production relations, and between the base and the superstructure—Premier Kim has stated that:

> . . . the successful building of socialism and communism depends largely on how socialist production relations are adapted to the ever-developing productive forces and how they are implemented; on how the superstructure is perfected to suit the foundations that are established; and finally on how the reverse influence of the superstructure on the foundations is to be stimulated (p. 111).

In North Korea, the author claims, such interrelationships have been formed correctly; consequently, there is "a great upsurge of socialist construction" because of the successful application and correctness of the new guiding systems of agriculture ("Chongsanri spirit and method") and industry ("Daean work system"). Both systems, which were created by the premier in his "on-the-spot guidance" while visiting a cooperative farm and a machine factory, emphasize political and ideological work above everything else, very much like the Chinese cultural revolution. "By giving precedence to politics," said the premier, "we mean doing the political work well first, then other work. When political work is done well, administrative [and other] work will naturally go well, too" (pp. 116–117, 134).

Recently, through the "Chollima movement," people in the North were subjected to the most intense ideological indoctrination ever experienced in the country's history. (According to legends in Korea, the term *Chollima* refers to a winged horse capable of bearing those fortunate enough to mount it at great speed toward the land of happiness. The movement was born in the North, in 1956, when the premier called on all workers to forge ahead at the speed of the Chollima horse.) Its essence, as explained by the premier, is the general line of the Workers' Party; that is, ". . . to educate all working people in communist ideas, to remold them and rally them more firmly around the party, and to give full scope to their revolutionary zeal and creative talents so as to build socialism better and faster" (p. 154).

In regard to South Korea, the premier believes that she is not only " a complete colony of the U.S. imperialists but also their military base for invading the whole of Korea and Asia"; hence, people there live in "a living hell where poverty and hunger, terrorism and massacre prevail." For these reasons, "we must force the U.S. imperialist aggressors out of South Korea and accomplish the unification of the fatherland, thereby fulfilling our national and international duties" (pp. 191–94). He therefore openly advocates the forming of a united front, the waging of a war of national liberation, and the unification of the country by the principle of *Juche*. Only when the U.S. imperialists are defeated, he says, will the world finally be safe for socialism and communism (pp. 290–303).

Throughout the book every effort is made to build up the personality cult of Premier Kim Il Sung. He is described not only as the official theoretician of Marxism-Leninism in North Korea but also as the only correct interpreter of building socialism in the transitional and developing world, following the footsteps of Marx, Engels, Lenin, and Stalin (pp. 158–189). Mao's name is never mentioned—a significant omission reflecting North Korean Communists' greater attachment to the Soviet Union than China, whether this attachment comes by choice or necessity. The book presents a simplistic, deterministic, and dogmatic theory of building a "better" society. It is not a scholarly book and was not intended as such. It is a propaganda piece—redundant, repetitious, and based on manufactured facts.

A reader of this review may wonder why it is necessary to pay serious attention to this book. There are three reasons: first, however distorted the picture of modern Korean society is, it presents the truth as many Koreans in the North believe it. This is a result of the systematic political indoctrination to which they are subjected; second, people in other countries may be easily swayed by its propaganda; and, third, it can be considered Premier Kim's *Mein Kampf*, a blueprint of action. Throughout the book the author relies upon the writings and speeches of the premier, especially his report delivered at the 20th (1968) anniversary of the founding of the DPRK. They are not the only references noted, but Premier Kim is actually writing in the third person. Hence, I suspect either that it was "ghost"-written by the premier or his closest advisors in Pyongyang, or at best that the author was authorized to write it as North Korea's unofficial propagandist abroad.

JOHN C. H. OH

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Kalahari Village Politics: An African Democracy. By Adam Kuper. (Cambridge: Cambridge University Press, 1970. Pp. ix, 191. $11.50.)

The village political system described in this case study of the Kgalagari people of the Kalahari region of western Botswana is fairly typical. The author tells us about the local politics of the Kgalagari and Tswana peoples of this region of the Kalahari desert. The system is made up primarily of the village headman and a council. Acting as a group, the headman-in-council is the main decision making body in the village. The author shows how the basic structure, which is part of the national system of local government, actually works from day to day. Using Eastonian concepts of demands and support in a modified input-output systems analysis, the author isolates the various forces that play upon decision making at the local level. He also shows how the national political system (primarily through the district authorities and now the Democratic party) intersects with the local village authorities. He indicates, however, that day-to-day political decisions, alliances, and changes are due primarily to internal distinctions of kinship, ethnicity, and personal political skills.

Except for the research site itself and the data on a hitherto poorly known area of Africa, not much is new here. All seems to operate as one would predict from what we already know of local politics in non-Western societies. The author knows his field sites extraordinarily well—therefore the quality of the data is high. But this reviewer was somewhat disappointed in the lack of theoretical or methodological venturesomeness. Instead of filling these gaps, the author chose to remain within the range of a small sector of British writings which allows his work to seem like a rather bigger frog in a somewhat smaller pond. Furthermore, his lack of theoretical ambition leads him to make odd conceptual statements here and there that might if more seriously considered have led him into more interesting interpretations. Thus, he equates the village constitution with political ideology (p. 169). Some thought about the lack of overlap of these two concepts could have produced a more original model of local politics.

All in all, however, good case studies on small remote corners of the non-Western world from a political perspective are still not everyday fare. Considering that most of mankind lives in such systems and has for most of human history, it is about time that a political science devoted to the politics of mankind took greater notice of such data. In this sense Kuper's book is a welcome addition to political anthropology which is in its turn an essential component of comparative politics and political science as a whole.

Ronald Cohen

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The Napoleonists: A Study in Political Disaffection. By E. Tangye Lean. (London, Oxford University Press, 1971. Pp. 402. $12.75.)

This is an unusual book. The jacket blurb warns that it is "not a conventional work of history but an investigation with original findings, which throws new light on the causes of disaffection in Englishmen." It is rather like an application of Erik Erikson's biographical-psychoanalytic methods to a diverse batch of people who did not form a cohesive group. Mr. Lean, according to the information provided by his publishers, appears to be an amateur both in history and in psychoanalysis. In the fine English tradition of amateurs treading where professionals might fear to go, he has produced a bold, uneven study, at times illuminating, often irritating.

The argument of the book is that among ten or twelve Englishmen and women who were contemporaries of Napoleon, a particular syndrome of background and character traits explains their admiration for the French emperor and their ambiguous attitude towards their own country and its government. The crucial elements in the syndrome are that these persons were "nonconformists, either literally, as members of Dissenting or Catholic families, or else by finding themselves from birth in a position which was equally shut off from the main body of the community," and that they were also unusually sympathetic to and involved in the American Revolution (p. 208). These two sets of experiences, however, were shared by a great many Englishmen, and Mr. Lean specifies that only a certain type of character, under these conditions, made a Napoleonist. The character type was marked by strong and ambivalent feelings toward the father, in which hositility was combined with love, loyalty, and even awe toward the strong father figure.

Mr. Lean asserts that a person of this type, under these conditions, tended to displace hostility onto George III and the government of England, and transfer love and awe to an authority figure outside the country, Napoleon. Further distinguishing principles that form a part of the syndrome include a taste for opposition, the tendency to set up a rival court, suicidal tendencies, love for travel and facility for languages, passion for acting but marked speech difficulties, love for animals, dandyism, and compassion for the unfortunate—mixed with a streak of violence and aggression. These traits, remarks Lean, add up to a certain resemblance to Napoleon himself, and indeed were shared by most Romantics. But only in conjunction with the two experiences—dissent and sympathy for America—do they make a Napoleonist.

The dozen subjects include Hazlitt, Leigh Hunt, Whitbread, Princess Caroline, Mrs. Inchbald, the Hollands, Byron, Fox, Godwin, and Hobhouse. In the last part of the book, Lean finds among another diverse group of six whom he calls "neo-Napoleonists" a striking repetition of the "syndrome." The modern six, who admired Hitler or Stalin rather than Napoleon, were Lloyd George, Oswald Mosley, Kingsley Martin, Kim Philby, Guy Burgess, and Donald Maclean.

A very mixed bag indeed, and a rather tenuous argument to hold the contents together. Since the second condition Lean originally specified (involvement in the American Revolution) can hardly hold for his six moderns, and the first condition (dissent) must be interpreted so broadly in order to encompass all his candidates that it comes to mean simply some kind of social exclusion in childhood, Lean's causal explanations are unsatisfactory. All these sixteen or so people had colorful or extraordinary fathers, but they reacted to them in different ways, and this hardly provides the kind of distinguishing feature needed to produce, as Lean claims in his preface, "a situation recurring in closely similar terms." Lean has managed to "throw light on the causes of disaffection among Englishmen," which is all his publishers claim for him, but he has hardly established "*a* syndrome," which is the stronger claim he makes for himself.

Lean's style is oddly oblique and mannered; his characters are often bloodless, and his narrative is hard to follow. Most annoying of all, Lean lists all his sources in general batches at the end, and uses almost no footnotes. This makes it hard to pursue his suggestive ideas and impossible to judge his use of his evidence. Such a method also cheats the reader of sharing in the discoveries of the author. We can only assume that Lean does not enjoy sharing the process of research with others, or that he took bad notes.

Lean says curiously little about the political views of his subjects, other than their attitudes towards the dictators. This omission considerably reduces the value of his findings for political analysis. Also, it is unclear how extensive a claim he is making about the applicability of his "syndrome" to other admirers of Napoleon for whom we have less information. He begins his book by casting a very wide net, and ends with a postscript on the nature of the English people. The focus of his argument shifts throughout the book, and therefore his claims are hard to evaluate.

Despite my frequent sense of annoyance with the book, I found times when Lean's characters seemed lifelike, his arguments were cogent, and his prose was admirable. He has assembled a great variety of information, and his sympathy for his subjects is not so strong that it overcomes his ability to criticize them. The book cannot be skimmed; it takes slow reading and a willingness to shift back and forth in order to evaluate Lean's evidence in light of later arguments. Read in such a way, the book offers occasional insights and some rather arresting portraits of interesting people.

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The Berlin Police Force in the Weimar Republic. By Hsi-huey Liang. (Berkeley: University of California Press, 1970. Pp. xviii, 252. $8.95.)

Can "city history" tell us more than the history of a city? Professor Liang thinks so. Although his study is conceived as city history, it is seen more broadly as contributing to the social history of modern Germany. His focus is the Berlin police force between 1918 and early 1933. Information for the study is drawn from interviews conducted by the author in 1962 with more than thirty veterans of the pre-1933 Berlin police force and from period literature on Prussian police theory and practice, unpublished documents (including court records), and Berlin newspapers. Secondary reliance is placed on a miscellany of background materials and commentaries. Excluding deliberately the administrative police, the study concentrates on the *Schutzpolizei,* the *Kriminalpolizei,* and to a lesser extent the *Aussendienst,* charged with investigating subversion and political crimes.

The study contains no surprising conclusions or new interpretations. It serves instead to document, carefully and dispassionately, the conclusion that the Berlin police force was more a pawn than a player in the game of street fighting and instant politics that both reflected and aggravated the political tensions within republican Germany. The study rarely conveys the atmosphere of high drama and acute frustration that characterized certain phases of the Weimar Republic, in Berlin and elsewhere in Germany. That it fails to do so is a mark of the author's faithfulness to his subject. With few exceptions the rank and file Berlin policemen (the level of focus for the author) were apparently more responsive to the values of legalism than those of republicanism, and if the policemen shared any perceived frame of reference, it was one of order, stability, status, and professional competence. Mostly reared in and recruited from the provinces and *kleinbürgerlich* backgrounds, they seemingly neither understood nor felt the need to understand the causes of the turbulence they were called upon to quell. Moreover, the perceptions they brought with them when they entered the service were not likely to be sharpened by the half-hearted civic education programs provided as part of the in-service training of Berlin policemen.

Early in his study Professor Liang asks "had the Prussian police in Berlin truly failed in its appointed task as a security force, and if so, how had it failed, and why?" A clear answer is not provided by the study and perhaps the question is even misdirected. A police force can serve, but it can neither create nor save, a community. Berlin of the 1920's, like the nation of which it was a part, was in search of a community. Why was it, and whose responsibility was it, that the search culminated in Hitler's accession to the chancellorship? The historian of pre-1933 Berlin can contribute to the answering of this question, but he will have to look beyond the city's police force.

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British Politics and European Unity: Parties, Elites and Pressure Groups. By Robert J. Lieber. (Berkeley: University of California Press, 1970. Pp. 317. $9.00.)

On his penultimate page Dr. Lieber quotes Stanley Hoffman's comment that in the search for European integration, the leaves of the artichoke (steps towards economic unity) have to be peeled away before the ultimate heart (the separate and intractable problem of political sovereignty) is reached. Dr. Lieber has himself produced a literary artichoke, in which the ultimate heart—the impact of pressure groups on the formation of Britain's European policy 1956–67—is surrounded by some distracting leaves.

The first leaf to be discarded is Dr. Leiber's account of Harold Wilson's flirtation with Europe in 1966–67. It reads like a footnote to his more extensive study of the earlier period, occupies only thirty-four pages, and is too cursory to substantiate the conclusions drawn from it in the last chapter.

The second leaf is Dr. Lieber's analysis of the opinion-forming role of the British press during the three periods when British entry into Europe was being actively considered, the 1956–60 Free Trade Area discussion, the 1961–63 Macmillan application, and the 1966–67 attempt by Harold Wilson. Newspaper comment is studied in some depth for the 1961–63 negotiations but only briefly mentioned for the two other periods. Even for the middle period, Lieber almost completely ignores the weeklies (*Economist, New Statesman* and *Spectator*), which were vitally important in determining British élite attitudes to the Common Market. Television coverage, which was crucial in shaping popular opinion, is not mentioned at all.

The last leaf to be peeled away is Dr. Lieber's examination of political party attitudes toward European unity. He concentrates on the two and a half main parties, Conservative, Labour and Liberal, (ignoring the Communist Party) but adds little to our knowledge; and he does not probe the most interesting question, which is precisely how and why the two reigning parties were so bitterly divided internally in their policies towards Europe.

Dr. Lieber's book would be more forceful and satisfying if he himself had pruned the leaves which obscure his main preoccupation—an examination of the attitudes of the Federation of British Industries (FBI), the Trades Union Congress (TUC) and the National Farmers' Union (NFU), to the European issue. From this examination he draws some interesting and original conclusions about the role of pressure groups in the formation of foreign policy.

The TUC he suggests, played very little part at any stage, even though it appreciated the importance of the issue earlier than the other groups, or even the government itself. The NFU was a major force in defeating the attempts to create a general Free Trade Area in 1956–58 by its insistence that agriculture be excluded from the proposals. Subsequently it limited Macmillan's freedom to maneuver in 1961–63 by insisting on safeguards for the British farmers but was less important in 1966–67. The FBI was most effective when it gave the European Free Trade Association proposals in 1958–60 an enthusiastic support which it had withheld from the wider Free Trade Area project in 1956–58. (Dr. Lieber does not mention that this change of attitude sprang largely from fear of West German competition in the FTA which disappeared in the more restricted EFTA.) During 1961–63, the FBI, like the NFU, was more important in prodding Heath in the Brussels bargaining than in forcing Macmillan to seek entry.

Dr. Lieber produces a neat summary; pressure groups were most powerful in 1956–60 when the issue was economic rather than political, less effective in 1961–63 when the issue had become partially politicized, and only incidental in 1966–67 when politicization was complete. This seems a misreading of the situation. The changes were not so much in the politicization of the issue as in the nature of the European organization facing Britain across the Channel, in the importance of the Commonwealth both economically and politically, and in Britain's economic power relative to the Six. These factors forced the Cabinet to seek entry and also changed the attitudes of the FBI and the NFU. The latter were less significant in 1961–63 and 1966–67 because they were less certain, and the government was more determined. The power of pressure groups varies according to the relative unity of purpose of the groups seeking influence and of the government framing policy.

Dr. Lieber's clearly presented thesis oversimplifies the story, since he has concentrated on examining the relations between the pressure groups and the government, and has paid too little attention to the divisions within the pressure groups themselves. The NFU's role is the easiest to assess because its members were fairly united in their attitude to Europe. The FBI and the TUC were internally divided, depending partly on their economic interests and partly on their emotional reactions to foreign entanglements. A significant study could be made of these internal divisions, how far they were based on self-interest, how accurately that self-interest was measured and how the divisions changed between 1956 and 1967.

Dr. Lieber has indicated many of the areas that need to be studied. One hopes that he may return to open the heart of this artichoke and tell us what he finds within.

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The Politics of Puerto Rican University Students. By Arthur Liebman. (Austin, Texas: University of Texas Press, 1970. Pp. 205. $6.00.)

Puerto Rico has long had an identity problem. The island was neglected by Spain except as a military outpost. The Catholic church failed to send enough clergy to serve the population so that many Puerto Ricans went unchurched. Roads were poor or nonexistent and there was little sense of community. As Liebman puts it, the development of indigenous institutions in Puerto Rico was weak. At the time of the U.S. occupation, about 95 per cent of the population was illiterate.

Under the aegis of the U.S., Puerto Rico has developed an extremely prosperous economy by Latin American standards, per capita annual income being $1400. But the economy is closely tied to the U.S. Liebman points out that its main "industries" are tourism and distributive outlets or assembly points for U.S. goods. Dominant American ownership means that profits flow heavily back to the continent instead of being re-invested in the island. The unemployment rate is about 13 per cent, with a high level of consumer debt.

Politically Puerto Rico is an "Estado Libre Associado" of the United States, a Spanish phrase which is more descriptive than the usual English translation of it as a "Commonwealth of the United States." Puerto Ricans elect their own Governor. They pay no federal taxes since they are not represented in Congress. On the other hand, they are subject to U.S. military service and receive U.S. military defense. They are free to come to the continent and once here, they automatically exercise the full rights of U.S. citizenship. The Island also receives much the same federal aid that is extended to the states.

Following the U.S. example and with U.S. help, Puerto Rico has established universal compulsory elementary education and has enrollments at the high school and university levels that are much higher, proportionately, than those of many wealthy industrial countries.

Puerto Ricans are understandably ambivalent about their unique relationship to the United States. During the long gubernatorial reign of Muñoz Marín and the Popular Party, a majority eschewed the alternatives both of independence—which would have meant a drastic decline in living standards—and of statehood, which also entailed economic disadvantages such as federal taxes. At present, the Statehood Party led by Governor Luis Ferré is in office. At the Rio Piedras campus of the University of Puerto Rico (UPR), Liebman's sample survey shows, the more than 18,000 students were politically divided in 1964 about as follows: 25 per cent supported the Statehood Party; 23 per cent were Independentists; 46 per cent supported the Popular Party; 2 per cent supported the Christian Action Party; and 3 per cent did not answer the question. At the same time, the student Independentist group, FUPI, had recruited only 2 per cent of the students to its membership. Although it claimed to accept anyone who supported independence for Puerto Rico, it was in fact, Liebman says, a far Left Castroite and Maoist organization.

As a people, Puerto Ricans are sometimes described as "gentle," sometimes as "passive." In the 1950's Alfred Kazin called his students at UPR "sheeplike." He was angrily attacked, of course, by Puerto Rican critics, but one of the leading critics, the playwright René Marques has himself written an essay entitled *El Puerto-riqueño Docil.* Liebman's survey of the University of Puerto Rico leaves a similar impression of rather apathetic centrist-conservatism. In a referendum in 1965, students voted two-to-one in favor of restrictions on campus political activity.

Liebman believes that the most important subsystem relevant to student politics is the colleges which the students enter after spending their first year in the Faculty of General Studies. Students in the colleges of Natural Science, Engineering, Business, Education and Medicine are the most centrist and least inclined to the left, while in the colleges of Law and Social Sciences a plurality are leftist; in the small college of Humanities the students fall between these two groups. This cross-sectional survey is unable to pin down whether the differences are due to a process of political socialization within the separate colleges or to differences in the kinds of students the colleges recruit, or both. My own 1960 data showed that the colleges in fact recruited students who differed a good deal in terms of ability and social characteristics, but the patterns of recruitment would not serve to explain the political differences Liebman found. All the professors of social science Liebman interviewed were independentists, whereas none of the nonliberal arts professors he interviewed supported independence. Surely this suggests divergent patterns of political socialization in the colleges.

Liebman devotes a chapter to FUPI, the student independentist group. Like its U.S. counterparts, it is a tiny minority—some 60 core activists and about 300 members—on a campus of over 18,000. Like its U.S. counterparts it recruits from among the most brilliant students. FUPI members are ardent in the cause and overwhelmingly committed to work for it. Although it is small, the Administration worries about it—and with reason. While they are not FUPI members, 23 per cent of the students favor independence for Puerto Rico. This is tinder enough for a conflagration.

In an epilogue Liebman briefly attributes the UPR disturbances of 1970 to increasing polarization of Right and Left due to the victory of the Statehood Party; to resentment of the Viet Nam war; and to careful study of U.S. student tactics by UPR students.

As I write, the UPR is still closed after a riot on March 11, 1971, in which two policemen and one ROTC student were killed and over 60 people were injured. The *Times* reports that Independentist students of the Catholic University at Ponce are planning a demonstration to demand that the government take over the private universities (such as theirs) and then turn the entire system of state universities over to student control. Thus, the U.S. student politics of the sixties has arrived at Puerto Rico after only a short delay.

The original material in the book consists of a cross-sectional sample survey of 577 students at the Rio Piedras campus of UPR in 1964. The questionnaire was devoted to their backgrounds, their university life, and their political beliefs and attitudes. The survey was competently done and the data well analyzed. It is not a very original study, nor was it meant to be. The main importance of this work, as its author suggests, lies in its being part of the Comparative Universities Project of the Brookings Institute and Harvard under Seymour M. Lipset. The cumulative significance of this project should be much greater than any of its parts.

Leila A. Sussmann

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Race and Class in Latin America. Edited by Magnus Mörner. (New York: Columbia University Press, 1970. Pp. viii, 309. $10.00.)

Catholicism, Social Control, and Modernization in Latin America. By Ivan Vallier. (Englewood Cliffs, N.J.: Prentice-Hall, 1970. Pp. x, 172. $4.95.)

Race, class, and religion—they are heady subjects, and old-fashioned ones. They smell of the last century, evoking the perfume of Marx and the early Marxists, and the concerns of Mannheim, Durkheim, Weber, and of Proudhon, Mosca, and Ortega. These books also bid us think of the long line of social encyclicals, and invite us into the world of Mediterranean social thought which most social scientists abandon after Machiavelli. Once back in this subject matter, we read nothing of systems analysis, game theory, mass-aggregate data analysis, or the search for general principles; but we are told of models, elites, social surveys, and how to infuse historiography with empirical theorizing. That is, these books treat of old subjects, but their authors are social scientists, and not essayists; they are interested in conceptual rigor as well as in the significance of their research subjects, and are unwilling to surrender either to the demands of the other.

Aside from the nature of their subjects and themes, and the cultural area of their concern, these two books have little in common. Ivan Vallier is concerned with building a theoretical frame to explain the evolution of Catholicism in Latin America and its relation to political and social processes of development. He devotes little space to descriptions of the organization and functioning of the Church in specific Latin American countries. Only one chapter is given over to this subject, and in it Vallier lightly touches on "influence systems and Church development" in Argentina, Brazil, Chile, Colombia, and Mexico—all in twenty-six pages, including a typology for the comparison of the five cases. He does not conern himself explicitly with social classes or races; neither term, nor any synonyms for them, appear in the index. But if he does not write of class in general, he is entirely clear in the emphasis he places on elites as the most powerful single element in a class structure. The word not only appears in the index with twenty-eight page references, but the concept is central to his definition of development. To wit, ". . . 'development' possesses a special analytical quality, pointing to the macroscopic changes that change-oriented elites bring about to increase the over-all capacities of a system to meet and deal with its major environments" (p. 13).

Thirteen scholars contributed to the volume on race and class edited by Magnus Mörner, a collection of papers prepared for a 1965 conference sponsored by Cornell and Columbia Universities. Necessarily, multi-authorship does not permit this book to have the unity of style and the tight reasoning that Vallier brings to his work. Still, it is one of the most coherent and best organized anthologies this reviewer has ever read. The book opens with three chapters on the abolition of slavery in Latin America and its aftermath, laying the scene for the interplay of Indian and Negro groups in Latin America. The next section deals with immigration, stratification, and race relations with special attention to Peru, the Dominican Republic, and Brazil during the last century. In this way European migrants are stirred into the *criollo*-mestizo-Indian-Negro mixture established in the first three chapters. The next section deals only with Indian problems during the nineteenth and twentieth centuries, turning to Indo-America in the national incarnations of Mexico, Peru, and Guatemala. The book closes with four considerations of the present state of knowledge and suggestions for future research, the subject to which we shall return later. Such a book cannot provide even a definition of class or race accepted by all the authors, let alone a common theoretical orientation or set of working hypotheses. Where Vallier is theoretical and uses example only for clarification, this collection is descriptive, and uses only that minimum of conceptual glue needed to make each national story adhere to its conclusions. Needless to say, the Mörner collection has many mentions of religious sects and the Catholic Church, but no organized consideration of religious beliefs or the institutionalized Church in its relation to stratification systems.

Vallier's principal interest for political scientists will lie in his analysis of political instability in Latin America. After a painfully inadequate glance in the direction of other theories of Latin political instability that ignores Marxist analysis and dependency theory, let alone the multitude of other partial explanations, Vallier states his "main assumption" as being, "Political development in complex, changing societies requires the presence of an underlying core of religio-moral norms, which need to be relatively differentiated from the routine give and take of politics. . . . Political instability in Latin America is bred, in part, by the absence of a durable religio-moral foundation within which political processes can be stabilized" (p. 43). In pursuing this argument, Vallier emphasizes the structure of the Church, and its ways and means of translating beliefs and perceptions into effective behavior. He reaches the conclusion that Catholic groups in Latin America cannot be the initiators of change, but rather can act effectively at "second and subsequent phases of modernization" (p. 158). That is, if the Church is able to respond well to innovations coming from political and other extra-religious sources, and to do so in such a way as to deepen the religious meaning of its activities as well as its autonomy from dependence for power on other institutional spheres, then it will be in a position to release "secularly-relevant resources for further development" and help facilitate change. Vallier's point, then, is that the Church must pay more attention to its entirely religious functions in order to create the autonomous bases of power that will permit it to choose among various moves toward modernization coming from without. In reaching this conclusion, he takes us through an extended discussion of development theory, Church history, and comparative analysis of a typological sort, disciplined at critical points with the reduction of his presentation to models.

I subscribe fully to Vallier's thesis that "religio-moral" norms are critical to development, and have myself advanced the idea of national community as the secular version of such a set of values. But the content of the normative system needs some spelling out if that system is to have relevance to the *kind* of development Vallier sees as fitting to self-sustaining growth. If religions are by definition partially committed to absolutes, it may not be impossible for them to espouse normative systems that include areas of secular relativism, but it is certainly difficult for them to do so. Specifically, is it possible for Catholicism to do so? What are the theological consequences of making some neighborhoods in the City of Man temporally free of the sacred ordinances of the City of God? If we assume the possibility of appropriate theological reinterpretation, how can the Church *politically* manage the establishment of a secular society in which non-believers and many varieties of believers could live under the same set of social commitments? To raise these questions is profoundly to compliment Vallier. He has written a thoughtful and fine-grained book that all serious scholars of Latin American societies will have to deal with for a long time. It is the most important work on the subject since the appearance of J. Lloyd Mecham's *Church and State in Latin America* in 1934.

Charles W. Anderson has provided the chapter of direct political analysis in the Mörner collection. He writes,

> . . . there has been little research by political scientists *into* the political properties of class and racial groups. Class and race are generally independent variables in political science research. Characteristically, they are factors to be taken into account in the explanation of something else. Seldom have the political properties of class and racial groups been the explicit targets of research (pp. 234–35).

Anderson is, of course, entirely correct in this statement, an exemplary reflection of a taut and highly intelligent essay. It does not excuse the negligence of Latin Americanists to say that matters of race and class have received very little attention of the sort Anderson calls for in *any* country. Marx himself advanced more than a dozen different definitions of class; Weber weakened to the point of incoherence when he attempted to consider the element he called "party" in his tripartite composite of social class. Most social surveys employ an occupation-income or a socioeconomic status index, or some other derivation of occupational and educational characteristics to stand in for social class. Many social scientists continue to doubt whether the concept is any good or, if useful, whether it is researchable. The ills of the many may be the consolation of fools, but Latin Americanists do have a lot of company in their difficulties in handling the idea of social class. Still, it is the first categorical cut Latin Americans make when analyzing their own societies, and thus at least the *idea* of social class is unavoidable.

Because the Mörner volume could not fully confront conceptual and methodological issues in an integrated fashion, it will not be the milestone that Vallier has written. To neglect it would be a shame, however, for it is a series of interesting and highly competent and varied prods to the reader to grapple with a subject that American social scientists have for too long failed to treat with the intensity required for an understanding of our own society, let alone Latin America.

Kalman H. Silvert

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Japanese Society. By Chie Nakane. (Berkeley and Los Angeles, California: University of California Press, 1970. Pp. xi, 157. $5.00.)

One of the most arresting facts about modern Japan has been the phenomenal social progress it has made in the postwar period: its miraculous social transformation and its ability to adapt smoothly in the rapidly changing world. Thus, students of Japanese society have never ceased to ask what makes Japan so outwardly different from the rest of Asian societies, what does Japan have that other developing societies do not, and above all what is the structural core of Japanese society that facilitates this smooth transformation into the modern world. Although Miss Nakane does not attempt specifically to answer these questions, she has offered a key (a source of intelligence and insight) to an understanding of the distinctive characteristics of Japanese society.

Most studies of contemporary Japanese society, and by implication most students of comparative society, have been primarily concerned with its changing aspects, drawing a dichotomy between "traditional" and "modern" elements and eager to designate the former as contradictory and obstructive to the latter. Consequently, Japanese society has been made to appear as if torn by conflict between elements inhibiting modernity and elements aiding social progress. What Miss Nakane has done in this short but outspoken volume, however, is to refute this kind of superficial observation about Japanese society. In order to portray the extent to which the two seemingly contradictory elements are integrated in modern society, Miss Nakane extensively examines individual behavior and interpersonal relations manifested in the entire spectrum of Japanese communities today, from traditional to modern and from rural to urban. In the process, the author concludes that not only have the traditional modes of human relations survived but they have also shaped much of the social life in Japan's modern sectors. And she successfully constructs an image of structural continuity in Japanese society—the "vertical organizational principle" in Japan's social life.

The traditional social structure of a complex society tends to persist in spite of great recent changes. Many of the social practices of America's great many subcultures are perhaps cases in point. The characteristic human relationship which bears pre-modern origin in Japan, too, has persisted in various strata of the contemporary Japanese society. In the many lucid examples cited, Miss Nakane illustrates how this "vertical organizational principle," imbued with feudal ethics, has continued to be the ideal guideline for social organization in Japan's modern-day industrial enterprises, governmental organizations, educational institutions, and intellectual and religious communities, as well. She further argues that not only has this practice not hindered the processes of Japan's modernization, but it has greatly increased the efficiency in social progress. The efficiency and the dynamism of Japan's modern-day business organizations, such as Mitsui, Mitsubishi, or Sony (see *Time* Magazine, May 10, 1971), perhaps owe a large debt to the traditional structure of "vertical human relationship" to which Miss Nakane is alluding in this volume. Her illustrations are convincing, indeed.

Because of the rapid pace of urbanization and industrialization and the vigorous influx of Western practices, Japanese society appears to have changed drastically, especially in the postwar era. But Miss Nakane concludes that the basic social grammar of Japan has hardly been affected and that it is in the informal social systems rather than in the overt cultural elements that the durable forces are to be found. What is implied, therefore, is a theoretical construct of an oft-stated but not frequently tested proposition: that traditional systems tend to be carried over to the modern sectors, and consequently tradition in any society is likely to play an important role in the modernizing process. Conversely, in order to understand the dynamics of Japan's modernizing process, one would do well first to understand the structural core of Japanese society which determines the shaping of Japan's modern institutions.

The subject of modernization has fascinated social scientists for quite some time. But most researchers have largely focused on social change rather than social persistence. In this regard, Miss Nakane's work represents a refreshing attempt, one that should inspire further inquiry into the role traditional systems play in other modernizing societies. With regard to Japan and other modernizing pluralist democracies, Miss Nakane's hypothesis appears to be reasonable. But it would be interesting to see whether her contention would be as valid if applied to an actively modernizing oligarchy, such as China, Cuba, or North Vietnam.

As a social anthropologist and a native of Japan, Miss Nakane is not condescending at all when she asserts that she maintains a distinct advantage in handling the nuances of this subject. She has chosen a timely and truly suitable topic of discussion and has cleared the air of a great deal of ambiguity about Japanese society. Perhaps even more than the two preceding masterpieces on Japanese social life (Ronald P. Dore, *Japan's City Life* and Ezra F. Vogel, *Japan's New Middle Class*), Miss Nakane's book advances our understanding of Japanese society.

Tosh Lee

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Soviet Social Policies: Welfare, Equality and Community. By Robert J. Osborn. (Homewood, Illinois: The Dorsey Press, 1970. Pp. 294.)

The notion of "convergence" between industrial societies of the state socialist type (characterized by central planning, nationalized property and a single dominant political party) and societies styled liberal-democratic, (having a free market, private property, multi-parties and an infrastructure of interest groups) has been widely, even exhaustively, debated. Professor Osborn tries to give a new dimension to this discussion by considering Soviet policy in three important areas where if "convergence" is occurring it ought to be very evident. Selected for description and discussion are:

> (1) security and the individual's claim to assistance and benefits as against the system which has made him wholly dependent upon its complex survival relationship; (2) the motivations which are offered to the individual to find the level and type of education suited to his capacities, to select appropriate and meaningful employment, and to remain committed to his work; and (3) the shaping of the physical environment of the city dweller in such a fashion as to promote meaningful social interaction and to furnish him with a sense of identity within his largely man-made environment (p. 5).

By personal interviews and by reading the Soviet press, Professor Osborn has succeeded in collecting a good deal of factual material on these topics which will make the book useful as a teaching tool. He describes rates of tax, levels of personal income, and preschool and boarding-school provision, and he explains how pensions are calculated. He estimates that about one-fifth of Soviet expenditures on education, culture, and scientific research are financed from enterprise profits and not the state budget. He further points out that medical care and housing may be privately purchased. But the section on welfare does not uncover the extent to which Soviet social policy is distinct from that of advanced western states. The similarity of income-related pensions and the availability of private treatment for privileged groups and the low pensions of others are mentioned. To bring out the impact of ideology on social policy, which is one of the aims of the book, Osborn might have focused on a different set of problems. He could have considered the extent to which society through the state recognizes its responsibility to the needy—the blind, the chronic sick, the unemployables. He could have discussed the significance of payment for medicines in the USSR, which is a burning issue in the politics of a free national health service in western Europe.

In his chapters on opportunity in choice of work and education, Professor Osborn adequately summarizes recent Soviet work on the motivation of children for different kinds of jobs, and he shows how this is related to the occupational background of parents. Despite the emphasis (derived from Marxism) given to labor and production in the USSR a considerable proportion of the school leavers share an aversion to manual work. The great mobility of workers between enterprises and the wide choice of occupation is contrasted with the image some writers (including Merle Fainsod) have of the regimentation of labor in the USSR. Many graduates who have three-year assignments, "leave their jobs and places of assignment with impunity before the three years are up" (p. 138). One might also make the point that "mass desertion of construction projects" (p. 139) by voluntary labor has similar effects on employers as strikes in western societies.

In the third topic of the book concerning the organization of the urban environment, Professor Osborn emphasizes the ways in which urban development proceeds relatively autonomously of government control and even contrary to plan and regulation. Population ceilings for cities have been ignored, control over the growth of industries and other institutes has not been effective, and zoning plans for urban development have been violated. "Communist" ideas on communal living have been jettisoned in favour of the microdistrict (having family apartments planned around community facilities and shops) which are common in western Europe. Here in my view Osborn gives too much emphasis to the views of Strumilin and others who in the 1960's sought to revive the commune-type apartment block with separate provision for children. The much celebrated *Dom novogo byta* (House of the New Way of Life) has not been utilized for experimental living but has been turned into a student hostel. In this respect communal forms of living are now much more advanced in the West and particularly in Sweden. On the other hand, it would have been useful to have had a study of town design comparing the way that land values under capitalism influence site utilization with the impact of bureaucratic control in state socialist society. The effects on urban development of the very cheap standard-fare public transport system in the USSR could have been compared to the more market-based costs in western countries.

Soviet Social Policies contains much useful information, but it ranges over too much ground. Its poor index does not facilitate ready reference. The general conclusion is that ideas and ideology have not been of great importance in shaping social policies, and in the light of the data provided this seems a reasonable inference. But more detailed comparative studies with a much stiffer theoretical and methodological backing are necessary if the impacts of cultural and ideological factors on social policy in various countries are to be disentangled.

DAVID S. LANE

University of Essex

Political Elites. By Geraint Parry. (New York: Praeger Publishers, Inc., 1969. Pp. 169. $6.00.)

The study of elites, long a central focus in political science, is enjoying even more attention today as events in the real political world challenge the pluralist assumptions of the discipline. The futility of contemporary mass protests against the Vietnamese war and the lameness of efforts to build clientele participation into poverty programs (symbolized perhaps by the proposed solution of "benign neglect") suggest both the disproportionate power enjoyed by formally legitimated political elites and, possibly, the validity of the *operational* defense of elitism. Perhaps the sociological thesis of functional stratification is correct in assuming, in the tradition of Mosca and Pareto, that elitist rule is a necessary condition of democratic politics. One wonders, however, how much worse off we could be, with less talented leadership!

In comparative politics, work such as that by Arend Lijphart indicates that a system of elite consensus, accompanied by limited mass participation, has insured stability in several multiparty states, possessing badly fragmented political cultures. One must wonder, parenthetically, how the empirical evidence of Austria, Switzerland, and the Netherlands could have escaped the attention of those who have assumed that two-party systems, fulsome rank-and-file participation, and integrated sociopolitical cultures are necessary conditions of political stability. But the main point is that some variant of elite rule has been shown to be operationally viable in systems generally regarded as democratic and stable.

Facts are often embarrassing, however, and it is precisely here that I find Geraint Parry's little book so impressive. Using a nice blend of traditional theory and recent empirical research, he brings an unusual detachment to what has often been a contentious subject, elitism vs. pluralism in modern democratic systems. Perhaps being outside the penumbra of academic careersmanship in the U.S., Parry has escaped the constraints which young scholars there may sometimes feel, and which, at times, seem to result in selective citing, including non-citing. In contrast, Parry reviews all of the evidence. His framework is provided by a survey of "classical elite theory" in which Michels, Mosca, Pareto, and Wright Mills contribute the main foundations for an analysis of minoritarian rule and the anatomy of elitist theory. Parry is well aware that differences in degree can amount to differences in substance, and nice discriminations are made among their claims. Mills's emphasis upon the institutional bases of elite dominance is contrasted with Pareto's focus upon the psychological determinants of elite roles. The political *immobilisme* of the isolated, atomized mass is contrasted with the tendencies toward coherence and cohesion among the elite. Yet, the many "real-world" degrees of elite consensus, competition, and domination are carefully analyzed. The extent to which elite ascendancy comes to rest upon mass acceptance, with a negligible reliance upon coercion, is correctly shown to be the essential thrust of the classical elitist school. The consensual style of elite control is, of course, most apparent in ethnically and linguistically integrated political cultures, yet my own research among Canadian political elites suggests, with Lijphart, that the theory of elite consensus is also relevant in fragmented, multi-party systems.

His theoretical foundation built, Parry turns to recent studies of community politics for empirical tests of elite theory. The work of Agger, Dahl, Hunter and Presthus provides the main bases for his discriminating treatment of such issues as the relative methodological merits of reputational and decisional instruments for identifying elites; the extent to which elites exercise power in substantively discrete areas; the extent of role interlocking among various categories of elites; and the conditions of "elitism" and "pluralism."

Much of this is not new, but all of it is done with a refreshing detachment. And this, in my own opinion, makes Parry's book the best overview of this whole issue available. No doubt, perfect disinterest is a myth, but this analysis provides a welcome contrast to the polemics which have too often marred American analyses of community power structure. I suspect this difference reflects in part the relatively greater extent to which British universities have found themselves able to retain an "ivory-tower" position vis-à-vis society, contrasted with those of the United States. Perhaps, too, the British tradition of deference toward authority may make it easier for scholars there to consider elitism dispassionately. Whatever the reasons, the results are impressive.

Parry concludes with an analysis of several variants of "democratic elitism," posed as an alternative to the polar extremes of traditional elitism and pluralism. Democratic elitism seems more elite than democratic, however, since it argues not only that rational decision-making requires that rulers must be protected from mass pressure, but that occasional participation in elections is about all we can expect, or need, from ordinary men. Surely, again, one empirical test of this thesis is: How wise and effective have the big decisions been under this system, which has, one supposes, been the ongoing style in Western democracies?

A second alternative, "democratic radicalism," is set against the elitist variant. Here, precisely opposite to the assumptions of democratic elitism, the state's encouragement of real participation by local groups and individuals is the central theme. An end to the dichotomization of the "political" and the "nonpolitical" is urged, with the potential result, for example, that one's workplace could become an instrument of political self-realization. At the same time, the barriers to participation stemming from the need for talents and expertise (back to functional stratification) are again noted. Parry shows once again how these varying concepts of governance rest ultimately upon different conceptions of man's nature.

Well-written, brief, yet broad and synthetic in its theoretical compass, this little book should be widely used.

ROBERT PRESTHUS

York University, Toronto

The Political Economy of Communist China. By Jan S. Prybyla. (Scranton, Pennsylvania: International Textbook Company, 1970. 605 pages. $10.25, cloth; $6.95, paper.)

The author is to be complimented for his very comprehensive and thorough description and analysis of the Communist Chinese economy during the past twenty years. The study is arranged chronologically, with two chapters devoted to the period of "reconstruction and reform" (1949–1952); three chapters on the First Five-Year Plan (1953–1957); one chapter each on the "liberal interlude" (October

1956–June 1957), the Great Leap Forward (1958–1960), the recovery period (1961–1965), and the Socialist education campaign (1963–1965); and two chapters on the Cultural Revolution (1966–1969). Even a review of the chapter titles would suffice to convince the reader that the Chinese economy has had many vicissitudes during the two decades of Communist rule. As the author points out, Peking's economic strategy has twisted and turned from one period to the next, and even within the same period it has not always been internally consistent. While a great deal of the development strategy of a given period is necessarily a reflection of the dominant ideology of that period, economic policy is also to a considerable degree a result of response to external exigencies. Consequently, there always are aspects of policy that look like compromise and that are otherwise somewhat incongruent with the spirit of the time.

Professor Prybyla points out from the very beginning then for Communist China, and indeed for all centrally directed economies, economics is essentially political economy, in the sense that one cannot separate the political background, outlook, intentions, and methods of the regime from economic policy and its implementation. In this respect the tabular material on pages 324–332, which contrasts the attitudes of the "radicals" with those of the "moderates," especially during the period of the Great Leap, offers a very useful guide to the student of Chinese leadership types and of policy in general, not just economic policy. For the same reason, chapters 9 through 12 should be intriguing to all students of Chinese affairs during the last decade. Chapter 11, which describes the political upheaval of the Cultural Revolution in great detail, may be a little too long, but even here the minutiae about the activities of the "rebels" and Red Guards bring home to the reader that political struggle and economic change under Peking's rule are not purely matters for dispassionate discussion in the drawing room or the planning office; their essence and implementation are inextricably interwoven with agitation, propaganda, and all the crudities of terror and rebellion. In these circumstances, one should not take idealized forms of economic development strategy for reality; the discrepancies are simply too gross to be ignored.

In chapter 13, the author speculates on China after Mao. He points to the deification of the individual and its deadening effect and suggests that Maoism may not long survive Mao (p. 573). It is also true, as the author points out, that the greatest obstacle to the long-term acceptance of Maoism is man's seeming inability to attain selfless perfection.

As for the plan itself, the nature of the Third Five-Year Plan, according to the author, was at issue before and during the Cultural Revolution; it probably never existed in detail, nor was it applied consistently (p. 486). With the arrest of economic planner and administrator Po I-po in January 1967, and the extension of the Cultural Revolution to the production sectors (p. 514), it is no surprise that economic controls collapsed. Thus one may question how these controls can be reconstituted and how the Fourth Five-Year Plan, which is to begin in 1971, can be expected to be implemented properly by a single central authority. This last question raises the possibility that the period of the Fourth Five-Year Plan may have to witness an altogether new strategy of development, namely, one that will be focused on the current defense buildup. Apparently the author concluded his work before evidence of the increasing Soviet military threat and Peking's comprehensive response to it had become unmistakably clear.

There are some specific points on which one can raise a dissenting voice. For instance, the author concludes, "Agriculturally and (barring certain sub-sectors) industrially, China in 1969 was not much better off than in 1957, . . ." (p. 573). While Chinese agriculture has made relatively little progress, this cannot be said of the sector of modern industry. Although estimates for 1969 based on information in the first part of the year may justify such an impression of little progress, the modern industry sector has recovered considerably for the whole year of 1969, and certainly in 1970. It is all too true, however, that the resilience of the Chinese economy has been taxed beyond all reason by "politically-induced business cycles." The strength of Communist China lies with its people and their hard work; its greatest weakness and hence the most stubborn obstacle to its progress consist of the inflexible policy and practice of some of its leaders and the revolutionary utopianism of others.

Another specific point perhaps worth mentioning is the author's discussion (p. 130) on grain output in the early 1950's. There is increasing evidence, as a result of recent research, that the statistics of sown acreage in China before the completion of collectivization contained substantial underestimates. This error therefore resulted in the underestimate of grain production in the early years more than in the later years, with the effect of exaggerating the growth rate of grain output. Still another area in which one would have wished to see more discussion is the effect of Research and Development on future economic growth and the question of whether the Chinese Communists are capable of learning from their mistakes so as to reduce economic instability. But one could always ask for more from any given publication. None of these comments should therefore be allowed to detract from what is a very good book.

YUAN-LI WU

University of San Francisco

The Himalaya Borderland. By Ram Rahul. Delhi: Vikash Publications, 1970. Pp. 157. Rs. 20.)

This study of the entire southern half of the Himalaya is segmented for analysis into the following seven area units: North-East Frontier Area, Bhutan, Sikkim, Nepal, Uttarakhand, Kinnaur and Lahul and Spiti, and Ladakh. Each unit is described in terms of its history, geography, ethnic composition, and developmental efforts under four functional chapter headings.

Rahul takes the view that "only by effecting proper socioeconomic development" in the heretofore neglected Himalyan borderland can India's national security be strengthened there against the dangers from China. In the chapter "Administration and Development," he examines the modernizing attempts in the Himalaya, giving valuable information particularly on India's efforts in its own border territories. It is unfortunate that the author does not deal with the theme more critically and in greater detail; instead, he devotes much valuable space to geographic and historical information which, although interesting, is already known.

Rahul's work is primarily a descriptive account with a minimum of political or economic interpretation and critical evaluation. Nevertheless, as a book of general interest on the Himalayan area, it offers useful data and descriptions for the lay reader.

KUNJAR M. SHARMA

Dalhousie University

Four Finns: Political Profiles. By Marvin Rintala. (Berkeley and Los Angeles: University of California Press, 1969. Pp. 120. $5.95.)

Professor Rintala's major concern in the book under review is with comparative politics, and he argues persuasively for a clearer understanding of the meaning of leadership in different political systems. Uncertainty about political leadership is seen in part as a reflection of insufficient information about individual leaders in many of the smaller political systems. Adequate data are a prerequisite for scientific generalizations, which must be the goal of the student of politics, but such generalizations are valid only so far as they are based on a knowledge of the political thought and action of individual human beings (pp. 5, 50). After citing with approval Harold Lasswell's comment that political science without biography is a form of taxidermy, Professor Rintala presents the reader interested in comparative analysis with political profiles of four Finns who helped shape the destiny of their country.

The first profile is that of Gustaf Mannerheim, one of the last great aristocrats in European politics who late in life was compared by a Frenchman to a member of the court at Versailles who had survived to the middle of the twentieth century (p. 37). The second politician to be analyzed is Väinö Tanner, who by virtue of his control over the Social Democratic Party is said to have been the most powerful single leader in twentieth-century Finnish politics (p. 48), a man who was committed to parliamentary government yet at the same time acted as an authoritarian in many political situations (pp. 65–69). The scholar in politics is K. J. Ståhlberg, the first president of the Finnish Republic, and a number of parallels are drawn between Ståhlberg and Woodrow Wilson, the former thought to be "one of the two or three greatest Finnish statesmen" and the latter "the greatest of all political leaders." But it is "a hot-tempered, foul-mouthed old banker," J. K. Paasikivi, who will be *the* hero for most Finns and who knew best what Max Weber meant by politics as a vocation.

Professor Rintala evaluates and ranks these four Finns—all but one of whom held Finland's highest political office—on the basis of three areas of conflict: linguistic conflict between Finnish-speaking and Swedish-speaking Finns, class conflict within Finland, and international conflict with the Soviet Union. In terms of domestic politics, Ståhlberg emerges the hero and Mannerheim the biggest loser. But in foreign policy it is Paasikivi, with his adherence to *realpolitik,* who shines; Tanner, whose personal responsibility for leading Finland into the wars of 1939–1940 and 1941–1944 is "greater than that of any other Finn," fares worst on this count. Because Professor Rintala is in basic agreement with Paasikivi about the primacy of foreign policy over domestic politics in the Finnish situation, Ståhlberg must remain second to Paasikivi in spite of the author's obvious personal preference for the former as a human being and statesman.

Professor Rintala is on firm ground when he states that the survival of Finnish independence and democracy is due at least partially to the excellence of political leadership in that system, and one must certainly agree that this "success story" need not run indefinitely. It is perhaps premature, however, to refer to a "bankruptcy of political leadership" and excessively pessimistic to feel that political leadership in Finland will never again be a stage for heroes. In particular, history may be more kind to President Urho Kekkonen than is suggested by Professor Rintala when he writes that for contemporary Finnish political leaders the personal possession of political power is more important than the use to which this power is put. It should nonetheless be emphasized that Professor Rintala's interpretive study is an exciting contribution to our knowledge of comparative politics and to our understanding of concepts such as political leadership.

JOHN H. HODGSON

Syracuse University

U.S. Corporate Investments in Taiwan. By Jordan C. Schreiber. (New York: Dunellen Company, 1970. Pp. xxl, 133, appendix. $9.95.)

This book, based on an empirical study, asks how much the expectations and motivations of American corporate investors coincide with the current and future aims of the government of the Republic of China. The author concludes that in the short run, for the next few years at least, the U.S. investors' objectives and the policies of the government are likely to be fully compatible. In the long run, however, the objectives and policies may begin to diverge with potential political and economic strife between the two sides.

At present, and in the near future, the policy of the Chinese government in Taiwan seeks to achieve three principal objectives: It maintains a low wage rate for its labor force. It supports the existence of a low real wage by its monetary and fiscal management designed to prevent inflation. And finally, it is committed to furthering internal political stability as well as economic stability and prosperity. These objectives are the very ones that foreign investors find highly conducive to good business in Taiwan. Schreiber's most important finding is perhaps that U.S. corporations, while certainly interested in profits, value very highly a low risk operation, and in fact appear to be willing to trade off profits for security. Unfortunately, Schreiber does not quantify the finding so that we do not know the lowest rate of profits which would be acceptable and consistent with perfect security, nor how the minimum profit rate would compare with that realized on domestic operations of the same type.

Of the seventeen corporations with equity investments in Taiwan included in the sample, sixteen appear to enjoy the benefit of AID guarantees that protect them against almost any conceivable form of loss. Other investors resort to a whole array of devices to minimize risk, ranging from low capitalization to formal agreements with the government to provide a virtual monopoly.

Yet according to the author, the very emphasis on reduction of risk, and particularly the assumption of that risk by such official U.S. agencies as AID, may cause trouble in the future. This trouble may come about because both the economic and the political objectives of the government are bound to change. The change in its political objectives will turn on such factors as U.S. actions within the United Nations and on the Vietnamese and North Korean issues. The change in its economic objectives is bound to take place as soon as the need for foreign capital and technology abates and domestic industry becomes more independent and more vocal. It is then that the U.S. investor is going to hear the familiar accusations that his company does not really introduce modern technology, that it reserves top managerial positions to expatriates, that it pre-empts the most lucrative segments of the economy, and that it impedes the growth of domestic industry. Inasmuch as these accusations lead to concrete government actions, the author tells us, American agencies will try to safeguard the interests of U.S. investors with possibly unpleasant political consequences.

These conclusions may sound plausible, but they are only partially derived from the actual data presented. The sample survey on which these conclusions are based consisted of some 20 interviews and 10 responses to mailed questionnaires, a total of 30 firms out of more than 200 U.S. businesses currently operating in Taiwan. The selection of the sample firms was done on a "judgmental" basis, and large size multinational corporations appear to have been emphasized. Moreover, both the interviewing and the mailing of questionnaires were done in the United States, rather than in Taiwan, with the result, for instance, that about half of the respondents had little or no knowledge of the conditions prevailing in Taiwan. In any case, no systematic information was collected from the Chinese side, so that whatever the validity of the U.S. responses, the attitudes and goals of the Chinese officials reflect the author's impressions, gleaned from conversations with ROC representatives in the United States.

Moreover, attitudes and beliefs are notoriously difficult to determine on the basis of either mail questionnaires or what the author calls "semi-structured" interviews. Indeed, there are indications that the respondents either did not think through their answers or else did not bother much about internal consistency. For example, two of the large companies operating in Taiwan but subject to U.S. federal corporate taxes indicated (p. 74) that low corporate tax rates prevailing in the Republic of China were "meaningful" incentives to operate in that country. Yet, of course, the local corporate tax rates are irrelevant to those corporations.

Nevertheless, the author's fears for the long run are justifiable for at least those U.S. investors who entered Taiwan either because the government directly requested it or because they sought to protect a local market. For the "offshore-sourcing" corporations which treat Taiwan as a production base for competitively priced inputs, with the assembly of the final output located elsewhere, the future seems more secure. These corporations (mainly in the electronics and instruments field) are becoming more important, even though the share of U.S. investment as a whole, measured as a fraction of all foreign investment in Taiwan, continues to decline.

The book suffers from a number of inaccuracies about conditions in Taiwan. Thus, Taiwan is far from having "one of the highest population growth rates in the world" (p. 3). Nor is it true that, in 1965, Taiwan's per capita income was the second highest in Asia (p. 15). The prevailing interest rates are substantially higher than the 11 or 12 per cent per annum mentioned on page 49.

ALEK A. ROZENTAL

Urban and Housing Development Committee, Taiwan

Heinrich Himmler—A Nazi in the Making, 1900–1926. By Bradley F. Smith. (Stanford: Hoover Institution Press, 1971. Pp. 211. $6.95.)

> I am referring now to the Jewish evacuation, the extermination of the Jewish people. It is one of these things that one enunciates easily. "The Jewish people is being exterminated," says every party member, "absolutely clear, stated in our program, elimination of the Jews, extermination, done." And then they all come, these good 80 million Germans, and each one has his decent Jew. Clearly, the others are all pigs, but this one is an extra fine Jew. Of all those who talk that way, no one has watched, no one has lived through it. Most of you will know what it means when 100 corpses lie there, or 500 lie there, or 1,000 lie there. To have gone through this and—apart from exceptions occasioned by human weakness—to have remained decent, that has hardened us. That is a page of glory in our history never written and never to be written.
>
> Heinrich Himmler to his SS generals,
> October 1943.

One morning in May 1945, an American unit in Munich was alerted to the possibility that the former Reichsführer-SS and Chief of the German Police might be circulating as a fugitive in the city. I was asked to follow any leads as to his whereabouts. Armed with the address of one of his relatives, I stepped into a well furnished home and asked questions about Heinrich Himmler. Yes, said the cousin, he had known Heinrich at one time, but in recent years the relationship was limited to an exchange of Christmas cards. No, nothing in Heinrich Himmler's childhood suggested even remotely that the mature man was going to do "such things."

The episode, all but forgotten, came back to me twenty-six years later, when I picked up Bradley Smith's *Heinrich Himmler.* Having read the author's earlier work on the young Adolf Hitler, I was quite interested in what Smith had to say about the upbringing of the man who was to serve as Hitler's principal killer.

The book is fascinating, absolutely engrossing, and I read it, text and footnotes, without omitting a word. Smith's careful reconstruction of Himmler's origins and youth generates suspense. Slowly he makes clear that Himmler had no special background, training, or problem. He was welcomed into the world, his childhood was not unhappy, his adolescence not filled with frustration, his college-age adulthood not burdened with want or deprivation. It dawns on the reader that the young Himmler may not be distinguishable from any intelligent and well protected youngster who grew up in southern Germany at the beginning of the century. His life in that setting was certifiably normal.

Heinrich Himmler was born on October 7, 1900, the second son of a *Gymnasium* instructor in classical languages. A Prince Heinrich of Bavaria was his godfather, and the elder Himmler rose to become a principal. The child's first years were spent in a comfortable apartment with a full-time maid. After measles, mumps, and lung infections, he was enrolled in a *Gymnasium,* where he performed near the top of his class. In the summer of 1910, he began to keep a diary from which Smith has drawn a great deal of information about the next fourteen years of his life. The first entries, brief, unemotional, and factual, were edited by Himmler's teacher-father. Perhaps, this tutelage made Heinrich a pedant—some thirty years later, as Reichsführer-SS, he corrected a report of a subordinate.

The First World War filled him with dreams of uniforms and glory, but on November 11, 1918, Himmler was still in the early stages of officers' training, without having had command or combat experience. This lack of fulfillment led him to seek all sorts of substitutes. Enrolled as an agronomy student, he joined a fraternity, and his spare hours were devoted to paramilitary formations created by postwar Bavaria in circumvention of Germany's disarmament. The years following the armistice were also a reading period for him. Himmler's own compilation of 270 books is reproduced in an appendix of Smith's biography. The list is eclectic enough to include, along with some racist authors such as Arthur Dinter and Houston Chamberlain, various works by Jules Verne, Henryk Sienkiewicz, Emile Zola, Fjodor Dostoevski, and Hermann Hesse. Significantly, Himmler's comment of April 1920 on Arthur Dinter's *Sin Against Blood* is guarded: though Dinter had presented the Jewish question with "shocking clarity," it was a "purely polemical novel, with anti-Semitic lectures."

At age twenty or so, Himmler was still an unfinished adult, undecided about his purposes and career. Holding on to his Catholicism, he still attended mass. He denied himself the pleasure of women and tried to toughen himself on a farm. For brief periods he studied hard for examinations and at times considered emigration. The desire for a uniform won out. Gradually forsaking other possibilities, he joined the *Reichskriegsflagge,* a quasi-military unit run by Captain Röhm, and sometime thereafter accepted employment with the National Socialists. By 1926, he was a complete Nazi, and three years later, at last in command of the party's SS, he wrote his erstwhile mentor in Bolivia that some day this organization would give Röhm "great joy."

Traces of Himmler's youth appear in several practices of the expanded and diversified SS: If the very young Himmler had to list petty purchases in a diary, the SS was required to account for every *Pfennig* in reports; if Himmler willingly endured physical hardship on a farm, the SS had to emulate his efforts in the field; if paramilitary formations in postwar Bavaria could be resourceful and self-sufficient, the same was expected of SS organs engaged in special tasks. Yet all of these elaborations of manner and style did not endow the SS with its fateful role in history. There is a difference, as Bradley Smith points out, between bureaucratic peculiarities rooted in personal experiences and drastic acts without precedent. Those acts, Smith finds, have no recognizable origins in Himmler's early years. His family, school, and 270 titles of books give no indication of what was to come.

Smith has shown conclusively that for Himmler (as also for Hitler) Nazism was an occupational choice. Neither Himmler nor Hitler really knew in their youth what they were going to do with power. Both spent years as dreamers and drifters. They spent additional years in the mundane business of building an organization for the seizure of the state. Only then did they start on the path of destruction, even as Germany's whole government and much of the German nation marched with them.

RAUL HILBERG

University of Vermont

The Process of Opposition in India. By Robert W. Stern. (Chicago: University of Chicago Press, 1970. Pp. 173. $8.00.)

A great deal of the research energy of political scientists studying the Indian political system has concentrated on electoral politics, political parties, and their struggles for power. The book under review provides a welcome shift in emphasis away from electoral politics and toward policy outputs—particularly the effect of policy on Indian politics. This shift follows the recent trends in American political science toward viewing policy as an independent variable that creates political actors, initiates group activity, and shapes attitudes and values.

The Process of Opposition in India is composed of just two case studies of successful opposition to Congress government policies during its period of dominance: the creation of a bilingual state of Bombay in 1956 and the attempt to control or limit the domestic manufacture and consumption of gold. To reconcile the paradoxical situation in which a dominant party is not after all dominant, Stern draws a distinction between the functioning of parties in opposition and the process of opposition as such. The process of opposition, according to Stern, is not a continuous function reserved to opposition parties so much as an intermittent process performed by interest groups and by subgroups within the dominant Congress itself as well as by opposition parties. Indian society and the Indian political system have a considerable latent capacity to articulate demands and compel decision-makers to take opposition seriously despite the apparent dominance of the Congress. This latent capacity, though only intermittently manifest, derives from the great number and variety of primordial, subnational, and economic groups which receive political expression not only through opposition parties but also through groups within the Congress which establish supportive relationships with opposition groups in response to specific Government policies. Despite Congress dominance, opposition has been successful because of the tenuous coherence of Congress (reflecting the heterogeneity of Indian society) and because of the very limited capacity of the political system to direct society or to compel acceptance of governmental policies.

Stern's case studies are not presented as successive, independent narratives; they are compared phase by phase in terms of background, interest groups, opposition parties, and Congress. Both cases, he concludes, demonstrate the structuring force of policy on politics. That is, politics was shaped by policy and not, as group theory would have it, the reverse. Policy was found to have been formulated in both cases after minimal consultation or after consultation only with senior Congressmen who were accepted as authoritative spokesmen for the interests involved. Latent opposition became manifest as a response to the policies of the Congress government elite. Since it was the policy that played a key role in calling forth and shaping the associational and group response, opposition to the specific policies of government was the group's *raison d'être,* and its life was coterminous with the threat of the policy itself. Even though the opposition succeeded in mobilizing and forcing a change or withdrawal of the policy, opposition as an organized entity failed to survive. As Stern puts it, "The shapes of Indian political arenas are amorphous, and the identities, strategies, capabilities, and even the existence of all the potential contestants may be unknown until a particular battle is joined" (p. 152). In a country like India, then, great constellations of organized interests do not exist at the national level. They are called forth by the battle and they demobilize after victory.

Stern's book provides valuable insights into the decision-making process in India and the pattern of interest articulation and group formation. At the same time, it suffers from several difficulties. In the first place, the book consists of only two case studies, which even the author admits do not exhaust the range of opposition-provoking issues or even the type of opposition they purport to illustrate. Nowhere does he explain why he chose the cases he did, or if, and on what grounds, his cases are representative of more general issues.

The two issues, in fact, are very different. The Bombay case necessarily involved strong primordial ethnic loyalties; the gold control case falls more into the category of socioeconomic policy. Since the Bombay issue was potentially far more divisive than gold control, the Congress leadership had to work out a consensus that could not only diffuse external opposition but prevent a split in their own ranks. If, as the author contends, the decisions on Bombay were reached within the Congress, largely as a family affair, then what after all was the function of opposition?

A careful reading of the Bombay case indicates that Stern depended very heavily on the account of T. R. Deogirikar, a member of the Congress Working Committee and President of the Maharashtra PCC, who was one of the active participants in the negotiations. We seem to be getting Deogirikar's perception of what happened, rather than a complete picture of all variables entering into the decision. Both cases, unfortunately, lack the richness of detail that can make the case study a valuable tool of analysis. Despite approximately a hundred interviews, the author seems to have drawn most of his material from public sources.

In spite of these difficulties, Stern's book points in a direction more of us should take. It does shed light on the difficulties decision-makers face in a political system where the associational infrastructure is weak or nonexistent, illuminating somewhat the role of public policy in shaping their emergence. Since the study of policy-making and policy output represents an important but neglected area both in comparative politics and in Indian politics, this book should be useful to students of comparative politics in general as well as to those interested in the problems of public policy in developing nations.

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No Exit From Vietnam. By Robert Thomson, (New York: David McKay Company, Inc., 1970. Pp. 224. $5.95.)

Among the multitude of books on Vietnam during the past decade, *No Exit From Vietnam* is distinctive largely because the author, Sir Robert Thomson, was a prime contributor to President Nixon's Vietnam strategy announced in December of 1969. Thomson remains as a "sometime" consultant to Nixon on Vietnam policy. The Thomson-Nixon version of "Vietnamization" was based upon three major points: (1) American troops should be withdrawn as South Vietnamese capabilities increase and North Vietnamese capabilities decline; (2) Remaining American troops must be properly balanced between combat and support troops as the withdrawal takes place; (3) South Vietnam must be built up as "a stable and viable country, not just a military camp" (p. 215).

Thomson has been a staunch defender of American involvement in Vietnam, but is critical of what he views as misconceived American strategy and misapplied American power. He contends that United States policymakers have not understood the true nature of People's Revolutionary (insurgency) Warfare and therefore could not react appropriately in strategic terms. Thomson's own background and experience was primarily in Malaya where, between 1946 and 1961, he held various posts related to emergency operations against Communist insurgents. From 1961 to 1965 he was head of the British Advisory Mission to Vietnam. In *No Exit From Vietnam* Thomson focuses on the years from 1965 through 1968, and sees some major American errors as bombing North Vietnam (thereby strengthening the determination and cohesion of the North Vietnamese); overexpanding areas of military operations and numbers of military personnel in ways that could have little predictable success; and offering to negotiate with the North Vietnamese rather than fighting a limited war to victory.

Thomson's long essay is essentially one of successive generalizations. It is in his lack of objective scholarship that fault can most easily be found. For example, on page 129 the author states that the Americans in Vietnam failed "to recognize that a government in any given set of circumstances can act beyond its current capacity and that in South Vietnam that capacity had been steadily declining." On the contrary, Americans all too well recognized the lack of capacity of successive South Vietnamese governments in the early 1960's, and were faced with the alternatives in 1964–65 of taking over the major share of responsibility for the war or leaving Vietnam to some kind of negotiated coalition government, including communist/neutralist elements. The severity of the situation was reflected in many official U.S. documents including, among others, a highly pessimistic *Resources Control Survey—Vietnam* (Saigon: Public Safety Division, U.S. Operations Mission, June 14, 1963). It can be concluded that, from 1963–65, any "middle-policy" such as Thomson's would have been highly unrealistic.

Actually, the concept of "Vietnamization"—in terms of calculated progress toward internal stability and ability to withstand external aggression—is hardly a new one. Though not called such at the time, "Vietnamization" was tried in the late 1950's and early 1960's, a period of relative peace and stability. But the "Vietnamization" policy of the Eisenhower and Kennedy years clearly failed. *If* Thomson's version of "Vietnamization" can succeed under Nixon, it will be at least partially because of the devastating confrontation of the American military with the North Vietnamese insurgents during Lyndon Johnson's presidency. It should be noted, too, that American policy of strengthening the governments of South Korea and South Vietnam was similar in terms of programs and spending during the several administrations from 1955–1965. Why, then, can South Korea be considered a success story and South Vietnam not? The answer is not to be found so much in "erroneous" strategies and programs as in the strikingly different potentialities of the two nations to take over the larger share of responsibility for their own affairs.

Another shortcoming of *No Exit* is the shallowness of its historical dimension which goes back at most 40 years. Vietnam's present problems have roots measurable in centuries, not merely years or decades. Thomson also fails to treat adequately questions related to Vietnamese intercultural receptivity to alien programs and procedures. This perspective is exceptionally well treated and documented by Nguyen Quoc Tri, professor at the National Institute of Administration in Saigon ("Cultural Receptivity to Induced Administrative Change—the Case of Vietnam," paper delivered at the Joint Meeting of the SEADAG Development Administration and Political Development Panels, Hong Kong, January 18 to January 21, 1971). It is difficult, too, to understand how Thomson can contend that "On the administrative side little if any assistance was provided to the Vietnamese in working out the structure required for efficient government of the country or in the training of qualified personnel to function within it" (p. 147). This is a sweeping condemnation of the U.S. A.I.D. program in the late 1950's and 1960's which, while perhaps less than successful, *did* entail substantial efforts to train indigenous government personnel. Among other things, a National Institute of Administration was established in Saigon for that very purpose. Finally, one must ask whether or not the American effort after 1964 was worth the staggering sacrifices made by the two Vietnams and the United States?

Thomson's successful experience in formulating strategy in Malaya's struggle against communist insurgency points to his competence as a military strategist. The more complex Vietnam situation, however, demands a considerably broader perspective than Thomson has offered. His narrowness of focus on little more than strategy tends to play down or bypass a wide range of other important considerations—historical, cultural, political, and international factors. Even so, *No Exit From Vietnam* demands attention based upon its author's influence on President Nixon's war strategy from 1969 to the present.

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The Changing Face of Communism in Eastern Europe. Edited by Peter A. Toma. (Tucson: The University of Arizona Press, 1970. Pp. 413. $4.95.)

This volume, in addition to providing an Introduction and Conclusion by its editor, Peter A. Toma, presents ten chapters by different specialists, each dealing with a specific aspect of East European politics. Eight of the chapters are concerned with developments in an individual country, and the remaining two deal with foreign relations with the USSR and the West. The volume comes out of an institute funded by the United States Office of Education, which was held at the University of Arizona. The list of contributors is impressive. The work will undoubtedly serve a very useful function as a beginning text in courses dealing with East European politics. It is less a specialist's work than an introduction to the study of East European politics with emphasis on contemporary developments.

Editor Toma and author Joseph R. Fiszman undoubtedly regret that the excellent chapter on Poland was written before the events of December 1970. The chapter is nevertheless remarkably perceptive in identifying some of the economic and political difficulties which led to the disturbances and the subsequent party changes. Mr. Fiszman takes the view that the changes in the higher party leadership which occurred in 1956 were really not very substantial, and he suggests a high degree of conservatism in the Gomulka leadership. The 1971 leadership without Gomulka, Kliszko, Spychalski, Strzelecki, Jaszczuk and others may represent a greater change, although, as in 1956, several of the former high command remained as a force of continuity. The now broadened base of support of the Polish Communist party should point to positive development within the framework of the Soviet alliance.

The chapter by Jan F. Triska on Czechoslovakia is based on interviews by the author in Czechoslovakia both before and following the events of 1968. It provides an admirable account of the breakdowns in the Czechoslovak economy and society which led to the proclamation on January 1, 1967, of the New Economic Model; the subsequent forces which it helped generate or stimulate; the fall of Novotny; and, finally in August 1968, the Soviet intervention. Triska identifies economic reformers, professional social scientists, creative intellectuals, students, and the Slovaks as among the principal forces for innovation. He contends (p. 176) that the main resistance came from the "non-professional, non-career, revolutionary universalists, the 'wise men' of the past, the agitators, the revolutionary heroes, the party organizers, the experts in ideology," who comprised a subleadership of the Czechoslovak Communist Party. Only reluctantly did the party conservatives see the need for a compromise solution with the innovators in order to solve the basic crises of the economy and society. The complexity and the broad scope of the changes of the New Economic Model increasingly taxed the capacity of the Communist Party of Czechoslovakia to maintain its traditional political control. The development of group pluralism and competitive interests and demands further weakened the scope and strength of Party leadership. According to Professor Triska (p. 189) the perception by Soviet leaders of "the Czechoslovak net gain in the spring of 1968 as becoming a Soviet net loss was based on a short-term calculation. Long-term stability and settlement in Eastern Europe suffered great harm."

Bennett Kovrig's chapter on contemporary Hungarian development, like Triska's' chapter on Czechoslovakia, places great emphasis on economic developments and the relation between economics and politics. The author argues (p. 202) that "a cautious pragmatism has characterized most of the Kadar regime's policies since the early 1960's." The regime has thus mitigated some of the sharp conflicts inherent in the system, and at the same time avoided the extremes of conservatism and innovation which characterized the Novotny and Dubcek regimes in Czechoslovakia. The New Economic Mechanism introduced in May, 1968, brought greater freedom to economic managers, additional power to the labor unions, and some improvements in agriculture and foreign trade. The Hungarian Communist Party has remained very firm in its alliance to the Soviet Union and very clear in the maintenance of the leading role of the Communist Party. At the same time, non-Party talent has been utilized and encouraged to display ideological correctness. Though the author sees some restrictions on this process of development following the Dubcek experience in Czechoslovakia, he points out that the system appears to be resolving some of its more important economic and political problems within the limitations required by its membership in the Soviet alliance.

The chapters on Romania, Bulgaria, the German Democratic Republic, Yugoslavia, and Albania maintain the high standard of competence which characterizes this book. Professor Fischer-Galati suggests (p. 23) that Gheorghiu-Dej's comprehension "of the incompatibility between Khrushchev's road to socialism and Mao's was his crowning political achievement." He proceeds to describe the Romanian New Course charted by Gheorghiu-Dej and by his successor Ceausescu in economics, internal affairs, and foreign relations. The writer sees the Romanian regime following the Soviet intervention in Czechoslovakia in 1968 as pursuing "a policy of fortification of its domestic and foreign positions."

Marin V. Pundeff's chapter on Bulgaria surveys Bulgarian developments in government, the judicial system, reforms, the economy, science, education, culture and arts, religion and national minorities. The thrust of the chapter is the underscoring of the political power of Todor Zhivkov since the consolidation of his power following 1962. The author argues that Zhivkov, like his predecessors, has based his power on complete acceptance of the leadership of the Soviet Union and the expectation that the Soviet Union would support him whenever that should be necessary. Within this framework he has appealed to Bulgarian patriotism and at the same time attempted to develop science and technology.

Donald D. Dalgleish, in his discussion of the German Democratic Republic, outlines carefully the various organs of party and state authority and shows the GDR second only to the Soviet Union in industrial output and attainment. His view of the future of the East German economy is, however, less optimistic.

Nicholas C. Pano's discussion of Albania, in addition to observing the Chinese-Albanian alliance, discusses the internal problems of the country, suggesting that the two major concerns of the sixties were the prevention of economic collapse and the elimination of all political opposition. The author suggests that the control of Hoxha and Shehu and the Albanian Communist Party has increased following the introduction early in 1966 of the Albanian Ideological and Cultural Revolution.

The excellent chapter by George Klein on Yugoslavia argues that the most vexing political problems of that country stem from its ethnic and nationality divisions. He points to a series of recent events which are central to an understanding of contemporary Yugoslav politics. Mr. Klein is clearly greatly impressed by the capacity of Yugoslav leadership for innovative and critical self evaluation. The changes and achievements of the Yugoslav political regime which he regards as most important include:

> (1) the decline of the command economy and statism and the general demythification of the role of the League of Comunists, (2) the freeing of the mass media from direct censorship, (3) restraint in the use of force in the exaction of political conformity, (4) the institutionalization of the politics of the several nationalities and the frank recognition that surface unity achieved by compulsion is undesirable, (5) a more benevolent style of life stimulated by a growing prosperity, and (6) the revitalization of the League of Communists of Yugoslavia (the League) based on popular decisions and external pressures (p. 218).

The volume concludes with two quite comprehensive chapters: one by Vernon V. Aspaturian, "East European Relations with the USSR," the other by Wolfgang Klaiber, "East European Relations with the West." These chapters are valuable summaries in which the authors show their skills in dealing with large subjects in small space. By contrast they point perhaps to a weakness of the volume. Except for these two chapters and the editor's *Introduction* and *Conclusion,* the book is essentially a country-by-country presentation with the format of each varying according to the individual author's interests. It does not provide a *comparative* overview of the East European socialist countries from the point of view of internal problems. In the absence of such comparison, the two foreign policy chapters are useful, but they seem to come as a sort of afterthought in the general scheme of the book. The volume does not replace standard comparative works such as those of Brzezinski and Skilling, but rather serves to bring the country data and analysis in those volumes up to date. On this score, Professor Toma has provided a most useful and serious contribution for the teaching and understanding of East European politics.

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A Century of Russian Agriculture: From Alexander II to Khrushchev. By Lazar Volin. (Cambridge, Mass.: Harvard University Press, 1970. Pp. 644. $18.50.)

With more than 540 million acres sown to crops the Soviet Union is one of the world's agricultural giants. Yet agriculture has been, and remains, the Achilles heel of the Soviet economy. Public pronouncements of Russian leaders—prerevolutionary and postrevolutionary alike—attest the crucial role of the agricultural problem, its economically and politically explosive nature, and its persistence over the years.

This is one of the most thorough studies ever made of Russian agriculture. Emphasizing the continuity of problems and policies too often dichotomized into tsarist and Soviet eras, Mr. Volin has created a valuable work—a panorama of the century between the emancipation of the serfs and the present.

The author, who served for many years as chief of the East European Branch of the United States Department of Agriculture, begins by recounting the development of serfdom and describing the emancipation and subsequent problem of land distribution. In the first part of the book he also explores the first agrarian revolution (1905) and the reforms that followed it, as well as the conditions during World War I that led to the 1917 Revolution.

In Part II he treats agricultural conditions during the Civil War, efforts to restore the economy by means of the New Economic Policy, Stalin's program of forced collectivization and liquidation of the kulaks, agricultural conditions during World War II—including Nazi policies in occupied territory—and the policies of Stalin in the postwar recovery.

The longest section of the book is devoted to the Khrushchev era. It covers capital investment and expansion of sown acreage, incentives for the collective farmers, their income, and the supply of consumer goods, as well as mechanization and electrification programs, the state farms, rates of production, and administrative control and planning. The final chapter summarizes the past century and comments on the outlook for the future.

Balanced judgment is one of the great merits of this volume. The author speculates about a fateful question of the Russian Revolution: why the sense of urgency about the agrarian problem, so characteristic of the liberal and democratic-socialist parties when they were in opposition under tsarism, was absent when they attained power in 1917. But he rejects the severe criticism of the Socialist Revolutionaries by scholars like O. H. Radkey, keeping in mind the central fact that the revolution occurred in the midst of a great war. A drastic redistribution of land, carried out during the war, would have had a detrimental effect on production, especially commercial production, and thus further strain the food supply. Equally important was the possible adverse effect of land distribution on the morale of soldiers at the front. It was feared that, no matter what the government assurances might be, the soldiers would suspect that in a general redistribution, their interests would be overlooked or disregarded. Against this possibility Volin weighs the dangerous state of the peasants' mood during the summer of 1917 and concludes "it is possible that a land reform, less disturbing to agriculture than a peasant revolt, might have been effected if the Provisional Government had acted early and energetically enough" (p. 123).

When explaining Stalin's radical policy of total collectivization in the winter of 1929–30, Volin avoids the mistake of taking either a doctrinal or power-political approach to the subject. Soviet strength, he points out, lay in the extent of recovery achieved during N.E.P., through nationalized industry, while Soviet weakness lay in the inadequate commercial grain production of individualistic agriculture. Stalin, as a Communist, was tempted to follow the collectivist path, with alluring promises of increased farm productivity, larger output, and effective control or subjugation of the farmer. But this policy did not automatically call for hasty, wholesale collectivization, brutally carried out. Indeed, as Volin shows, Stalin had to distort grossly the teaching of Engels that ruling socialists must be circumspect in their land policy. Volin offers some educated guesses about Stalin's failure to adhere to the gradual collectivization course laid down in the First Five-Year Plan, adopted under his leadership in the spring of 1929. Here Volin is less definitive than Moshe Lewin, the foremost Western analyst of collectivization, rightly admitting that there are insuperable limits to our ability to penetrate the iron curtain drawn over decision making in the Kremlin during the Stalin era.

The portrayal of Khrushchev is judicious and useful at a time when some commentators misrepresent the departed Chairman as little more than a foolish improviser. Khrushchev emerges from these pages as a bold, dynamic, realistic, if flamboyant, leader—one who knew what makes the Russian peasant tick. His zeal for technological progress and innovation was largely instrumental in dispelling much of the scientific isolationism so marked under Stalin. Volin demonstrates too how Khrushchev understood that without the help of economic calculus, without balancing returns and costs, rationality and efficiency could not be instilled into the management of Soviet agriculture, heretofore preoccupied largely with physical targets. At the same time, Volin points out Khrushchev's other traits, such as impulsiveness, impatience, lack of scientific education, cocksureness, excessive optimism, and a willingness to gamble for large stakes. The major questionable schemes espoused by Khrushchev are found to have been worthwhile; the fault lay in the means adopted or the manner in which they were pursued.

What may be dubious is the assertion that there was no question of decollectivization under Khrushchev. On the eve of his ouster, the premier favored division of the collective farms into small sectors that would be permanently alloted to workteams capable of measuring individual performance and distributing reward according to output; he also advocated granting farms the right to market a sizable part of their crop in the towns on a commercial basis—in essence, *de facto* decollectivization. Brezhnev, to be sure, has not given his blessing to these proposed changes.

We might regret Volin's failure to cite the tsarist practice of grain requisitioning in 1916 as a precedent for the Stalinist solution of the grain marketing dilemma. Nor does he discuss the fluctuation of Stalin's postwar agricultural policy: Ukrainian collectives in 1948 had to sell to the state 124 tons of wheat to buy one "ZIS-5" truck; just one year later the same vehicle cost them no less than 238 tons. This anti-peasant shift in the terms of trade between town and country is attributable to a massive diversion of resources to the armaments industries in response to Stalin's perception of a military threat on the Western borders of his empire. But any regrets have to be eclipsed by admiration of a masterful survey.

SIDNEY I. PLOSS

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Development Administration in Asia. Ed. by Edward W. Weidner. (Durham, N.C.: Duke University Press, 1970. Pp. xxiii, 431. $12.00.)

The thirteen essays in this book were prepared for one of the summer seminars of the Comparative Administration Group, this one at the East-West Center, University of Hawaii, in 1966. The collection is distinctive in presenting the work of six Asian scholars with that of their American colleagues, all focusing on the same theme.

The editor contributed the first and the last chapters. Between these are two essays on theoretical concepts and seven reporting research and summarizing situations in South Korea, the Philippines, Japan, India, Pakistan, and Micronesia. Two others deal with technical assistance in public administration.

To this reviewer the more important research reports are those of José Veloso Abueva and Bernard S. Silberman. Abueva's study of Filipino administrative culture and behavior is based on questionnaire responses of a rather small "sample" of 52 middle-level civil servants. Although the author expands and elaborates his interpretations with impressions and observations not directly supported in the data presented, his conclusions persuasively depict the interpenetration of "traditional" elements of Filipino culture and contemporary administrative behavior. At the same time, his data do show (p. 175) that a significant minority of "disentangled" administrative officials reveal "modern bureaucratic" characteristics. These, he suggests, will be the agents of further administrative development.

Silberman examines the background of a random sample (25 per cent) of prefectural governors in Japan between 1868 and 1945. He shows that after the Meiji Restoration and until around the turn of the century, membership in the governing class was a basic ascriptive qualification for recruitment and advancement. But within this group, participation in activities leading to the Restoration (and secondarily, possession of Western knowledge or education), characterized a very high proportion of those appointed as governors. By the 20th century, however, criteria of eligibility had shifted to legal training. Silberman suggests that the traditional elite origin of officials before 1900 was important in legitimizing innovative reforms. The uniformity of legal-generalist training (particularly between 1900–1920) institutionalized achievement-oriented criteria, but it also led to conflict with the specialists increasingly recruited after 1920. This conflict he sees as contributing to the declining effectiveness of civil bureaucratic decision making between 1920 and 1945. Silberman's chapter is a useful demonstration of the potential that quantitative analysis of historical data has for the study of administrative and political development.

Other chapters in the volume also make good use of quantitative data for analysis. For example, Hahn-Been Lee's study of the Korean higher civil service shows significant relationships between administrative and political change. Norman Meller, although he focuses on the Congress of Micronesia, presents very interesting data on the emergence of a new, modernizing elite in the Pacific Trust Territory which has fairly direct relevance to development administration.

The chapter on India (B. S. Khanna) and

the two on local administration in Pakistan (Harry J. Friedman, Inayatullah) are informative and insightful essays that make less use of quantitative data. The same is true of those on technical assistance (Shou-Sheng Hsueh, Nguyen Duy Xuan). These five chapters include evaluations of the relative success of efforts described and offer prescriptive admonitions.

In the two theoretical papers, Fred W. Riggs and Martin Landau each continue their customary, patient, yet often imaginative efforts of conceptual clarification and innovation. Riggs characterizes the core of development administration as the exercise of increasing autonomy, in the sense of ability to choose among alternatives. But he finds it necessary to distinguish between autonomy as "discretion" (independence of a role or system from its environment, i.e., "time-space setting, the characteristics of the actors involved, and cultural elements which affect a role") and as "autogeny" (independence of a role or system from all other roles or systems impinging upon them). The basic proposition then becomes that "although development involves an increase in the degree of autonomy (discretion) of social systems, it also involves a decrease in the degree of autonomy (autogeny)" (p. 36). Put another—and less precise—way, development involves both increased power and increased interdependence. This is by no means all that Riggs has to say in this stimulating essay, but it points to one of the chief virtues of his work: the effort to be clear in specifying just what he means by the words he uses. In some of his essays this has resulted in burdensome complications, but the present one largely avoids this.

Landau's contribution is his adaptation and extension of Thompson and Tuden's matrix for the classification of decisions, in which the analytical components are the dichotomous attributes of agreement or disagreement of decision makers on causal relationships (facts) and on outcome preferences (values). Crossing these categories produces a four-cell matrix in which decision making can be classified as "programmed," "pragmatic," "bargaining," and "inspirational." Landau's main contribution is to apply the matrix to predominant patterns of decision making in two polar types of societies: the "folk" and the "urban." Both of these tend toward "programmed" decisions—but for different reasons. Unfortunately, sustained attention is not given in this essay to the societies between the poles of "folk" and "urban." Such attention might have led to a more elaborate treatment of the "inspirational" category. This essay in two modified versions has also appeared in *Comparative Political Studies,* 1 (July, 1968), and in James B. Christoph and Barnard E. Brown, eds., *Cases in Comparative Politics* (Boston: Little, Brown, 2nd ed., 1969), both of which are worth reading.

Running through most of these essays and studies is the theme of "innovation"—its indispensability *and* desirability—for development and its administration. This is especially true of Weidner's concluding chapter, in which a whole series of role-types is predicated on their relation to innovative behavior. But before we install innovation as the supreme value of development administration, a word of caution may be appropriate. Innovation is not synonymous with development nor with its administration, although certain kinds in certain circumstances may be necessary if not sufficient conditions. Developmental agencies, having "broken through" with successful innovations, may have to stabilize and institutionalize their new patterns of action if they are to continue to be effective. At this point further innovation may be dysfunctional. The question, of course, is still which and when, and the best answer so far is, "it all depends. . . ."

This is a useful volume, often a very good volume, for students of what, to the deteriment of the English language, has come to be called "development administration." To criticize it fundamentally, one must go outside its frame of reference, and indeed raise a question about that frame of reference. First, the reader might ponder the absence of chapters on Asian Communist nations. Administrative problems of development in these political systems have been no less serious than in those around the Asian perimeter, and they are not altogether unresearchable. I raise this question merely to suggest that, from a scientific point of view, it would be unfortunate if the concept of development administration should acquire an ideological hue.

Finally, the political tensions (and their administrative consequences) resulting from the war in Indochina, and more broadly from international involvement in the developing nationalisms of all of Asia are only muted themes in most of these essays. Surely these events and forces cannot be relegated to the background in thoroughgoing analyses of "development administration in Asia."

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The Politics of Pakistan: A Constitutional Quest. By Richard S. Wheeler. (Ithaca: Cornell University Press, 1970. Pp. 346. $11.00.)

This book is one of a series of "broad guides" to South Asian political systems which will also include volumes on Nepal, India, Ceylon, and Afghanistan. Professor Wheeler has narrowed his focus to a survey of "domestic constitutional issues" and has thus excluded any direct analysis of the impact on political maneuvering in Pakistan of such factors as Kashmir, relations with India and China, membership in the Commonwealth, or foreign assistance. He notes that these topics are of "great importance" but holds that they are "peripheral to the primary constitutional dilemmas" (pp. xi–xii). Within the limitations of this framework, this volume is perhaps the most inclusive introductory description of Pakistani politics generally available in the West. It contains a large amount of information about people, organizations, and events and should prove a useful reference for the American student who wants an overview of the development of Pakistan's political system from 1947 to 1970. The book begins with capsule summaries of the origin of the nation and its social and economic characteristics. Its major portion consists of five chapters examining constitutional dilemmas, institutional continuity, institutional experimentation, parliamentary politics (1947 to 1958), and presidential politics (1958–1969).

Professor Wheeler asks the right questions: Why Pakistan? What distinguishes East from West? What is the role of Islam? What are the institutional contributions to stability and instability? What is the future of parliamentary or presidential systems in a nation without "disciplined political parties based on shared values"? The issues that have produced the 1971 civil conflicts are identified, and although that event is not predicted, its occurrence is not inconsistent with Wheeler's analysis.

Specialists in particular aspects of Pakistan's political system will find errors of fact, but these are usually the result of attempts to generalize about the political machinery of a nation that loves the exception. While describing Basic Democracies, for example, he erroneously asserts that "in 1962 the law was amended to eliminate appointments at all levels" (p. 150), and that the "chairmen of union councils, union committees, and town committees within the district elect the nonofficial members of the district councils" (p. 151).

On a broader level, Professor Wheeler's treatment of the relative importance of political parties in Pakistan's decision-making, conflict-resolving framework, and his avoidance of the political implications of poverty raise questions of interpretation. It may seem unusual to charge that too much space has been devoted to a discussion of political parties in a book about a nation's politics, but that emphasis must be suggested. Wheeler examines his subject within the framework of some rather traditional political science variables—pressure groups, legislatures, executives—but it is quite possible that the *politics of Pakistan* exist elsewhere. He is fully aware of this, however, noting at several points that the party system does not touch the lives of many people and that, indeed, it is the bureaucracy, not the political system, that usually resolves demands advanced by individuals and interest groups. The civil administration receives only summary treatment, however, and the uninitiated student, for whom the book is intended, may be misdirected by the attention given to the activities of the Muslim League, the Council Muslim League, the NDF, the Awami League, the Jamaat-i-Islami, and other parties.

My other major objection to Wheeler's treatment of constitution building in Pakistan is that he fails to focus on the deep, lasting, and ubiquitous poverty of the nation as it affects the political process. Issues related to inter-wing parity, regional autonomy, communal differences, language, and traditional vs. modern values certainly contribute to the character of politics in Pakistan; but they may all be overshadowed in the development of behavioral patterns by an urban poverty which has grown worse in spite of general economic advances and by the seemingly unsolvable economic problems of East Pakistan. Wheeler does report that "the great majority are concerned with the conditions of their livelihood and with securing justice from the local administration, and have little interest in or comprehension of the form and policies of governments" (p. 301), but he does not tie the politics of poverty to constitutional politics except to note that the organized opposition during the late 1960's gained support from the economically disadvantaged.

These comments are not intended to detract from the merits of the book. Instead they are inserted as *caveats* to the uninitiated who might otherwise view Pakistani politics as just another system of Western political parties and pressure groups. Although it may be a mark of the profession's growth, it is unfortunate that the readily available major studies, by both national and foreign authors, tend to measure Pakistani politics against essentially Western standards. One person to suggest other possibilities was Ayub Khan, and in his *Friends Not Masters: A Political Autobiography* (1967) he recounted his desire to create a pattern of institutions based on indigenous rather than European characteristics. He had a decade to implement his ideas, and he directed his country to a period of aggregate economic growth, but stability eluded him as it had his predecessors.

Both the man and the regime emerge rather well from this book. Ayub is depicted as a leader with ideas and dedication who became imprisoned "within the bureaucracy, in part from the failings of the Muslim League, which deprived him of alternative channels of information, and in part from his estrangement from the political opposition, which prevented his heeding and evaluating their criticisms correctly" (p. 302). The failure of Ayub's constitutional government and the re-introduction of military rule in 1969 emerge as almost natural consequences, and Professor Wheeler quite rightly asks: Is martial law a normal or natural consequence of Pakistani politics? He suggests the intriguing possibility that the martial-law regimes are revivals of the viceregal past, imposed and accepted because in 1958 and even in 1969 the society was not "sufficiently coherent to be able to work out its problems within a given constitutional order" (p. 314). In its third decade, Pakistan is still trying to find answers to questions posed in 1947.

GEORGE M. PLATT

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The Sōkagakkai and Mass Society. By James W. White (Stanford: Stanford University Press, 1970. Pp. 376. $12.95.)

Here is Herman Kahn telling us that Japan, the emerging superstate of the 21st Century, is going to outstrip the West in both gross and per capita GNP by the year 2000. We read of Japan's growing military strength. More yen for a bigger military each year. While documents have been signed, we certainly haven't heard the last word on Okinawa. Wherever Americans try to sell overseas, they discover that the Japanese already control the market with a cheaper but better product. Indeed our old fears about the Peril (Yellow) are being renewed daily as we turn our attention away from the Southeast Asian nations that we have never really had any reason to engage in conflict, and regard our old enemy and rival, Japan.

And what about that fanatic new religion that is sweeping the country, the Sōkagakkai? Considering the hatred and fear we generated towards "Shintoism" as the bulwark of Japan's quest for a Greater Southeast Asia Co-Prosperity Sphere in the 1930's and '40's, is it any wonder that we see the Sōkagakkai mainly as a rebirth of what we imagine to be the traditional, militarist, and authoritarian predispositions of the Japanese? Not everyone who has previously written about the Sōkagakki has chosen to pin such labels on it, but most have, and if you asked the average partially informed layman who has some interest in Japan to tell you something about the Sōkagakkai, he probably would reply with those adjectives and others which clearly indicate that somewhere he has learned that the Sōkagakkai connotes evil in organization and intent.

What the Sōkagakkai has needed for so long a time it has finally gotten in the form of James White's *Sōkagakkai and Mass Society*—a careful, theoretically-informed, analytical, and comparative study of the Sōkagakkai within the context of Japanese society.

White carefully and accurately describes the postwar Japanese environmental situation into which the Sōkagakkai was born (the Sōkagakkai was conceived before the war, but its major impact and real social significance occurred only afterward): Japan was utterly stricken by its crushing military defeat. All traditional values were suspect, though the majority of the heavily socialized citizens still could not avoid harkening to them. What values could take the place of the obviously discredited traditional ones ("Sōkagakkai" means "value-creating society")? The population was undergoing a massive dislocative shift from the tightly-knit countryside to the crowded and anomic cities. Could the Sōkagakkai develop an integrating structure which would take these lonely urban-dwellers and make them happy, productive citizens? The Japanese enjoy probably the highest rate of literacy in the world. Their mass media are extraordinarily pervasive and capable. Could the Sōkagakkai capitalize on the existence of this literacy and the media? Regardless of the fact that predispositions toward traditional value-preferences lingered, most Japanese yearned to be members of a modern, democratic nation. Never having been a "religious people" in a recognizable Western sense—our misunderstanding of the wartime role of Shintō to the contrary notwithstanding—could a new-old religion appear to be modern enough to become the largest voluntary organization in modern Japan?

These are some of the problems which White considers. The Sōkagakkai is an extremely well-organized and well-led secularized organization affiliated with the centuries-old Nichiren Shōshū sect of Japanese Buddhism. Its beliefs are—to most outsiders—not strikingly different from those of many of the other new religions which came to flourish in what McFarland and others call "the rush hour of the gods"—that period of frantic search for new values before the Korean War gave Japan the boost which enabled most Japanese happily to center their lives around essentially economically focused goals.

The success of the Sōkagakkai—it is the largest group of any kind in Japan—seems to lie in the peculiarities of the Japanese social system, and in the Sōkagakkai's own combination of traditional appeal, extremely capable and devoted (but not charismatic) leadership, forceful and untypical conversion techniques (especially the vigorous *shakubuku*), and a superb internal structure which keep the members busy, happy, and totally ensnared within a web of interacting and encompassing activities always under Sōkagakkai direction.

White devotes two very interesting chapters to the Sōkagakkai's political activities. The political party affiliated with the Sōkagakkai, the Kōmeitō, has utilized Japan's multi-member electoral district system with consummate skill so that it has become Japan's third most powerful political party, behind the permanently ruling Liberal Democratic party and the Socialists. In some local assemblies, indeed, the Kōmeitō ranks second in number. White describes its vague and essentially opportunistic platform and makes clear that its electoral success is not due primarily to any unusual ideological stance, but rather to good electoral techniques and studious attention to local issues and "clean politics" ("Kōmeitō" is frequently translated as "Clean Politics Party").

In Part Two, White analyzes the Sōkagakkai according to criteria generated from Kornhauser's theory of mass society to see if the Sōkagakkai can be considered an organizational response to the problems of mass society. White's clear, and I believe correct, conclusion is that the average Sōkagakkai member cannot correctly be characterized as a "mass man" according to Kornhauser's model, and the Sōkagakkai itself cannot be classified as a "mass movement." To the contrary, and without in any way over-glorifying the Sōkagakkai or espousing its beliefs, White concludes that within the specific context of Japanese society today, the Sōkagakkai is a socially and personally functional unit, far more positive than negative in its contributions to Japan and to individual Japanese citizens.

Unless major currently unforeseen alterations occur in Japanese society or the Sōkagakkai itself, White does not see the organization becoming a negative element in Japan. But it does have problems, for the most part associated with the "one-man" rule of its present President, Daisaku Ikeda, and the possibility that its large size may eventually rent its seamless robe with the emergence of factions.

James White is to be strongly commended for this book. It is well researched and well written, and stands as a major contribution and model for those who are interested in continuing the study of one of Japan's most fascinating sociopolitical phenomena.

JAMES ALLEN DATOR

University of Hawaii

Italy Chooses Europe. By F. Roy Willis. (New York: Oxford University Press, 1971. Pp. 373. $8.50.)

Roy Willis has written a very good book indeed. It is one-third history of the development and evolution of official Italian policies on the uniting of western Europe. It is two-thirds discussion of the economic, social, and political forces inside Italy which operated to produce those policies. The reader not only learns what Italy's policies were and who made them but he gets at the same time a short course in Italian economic geography, political parties, the role of interest groups (industry, agriculture, commerce, labor, the Church). Brief but accurate portraits are given of Italy's major problems of the postwar years—overpopulation, unemployment, emigration, disparities of economic levels among various parts of the country. All these lead up to an overview of postwar Italian society and politics, focused toward the consequences, realized or hoped for, of the choice to enter Europe. His conclusion is that in going for Europe the Italians have not been able to escape deciding what kind of Italy they want, a decision they still have to make.

Willis is quite accurate and the book is thoroughly researched. It has some deficiencies, however, which deserve mention. It lacks a real concluding summary of the author's judgments of the experience. As a result one has to guess at some of his opinions. He is quite explicit that Italy has paid a high price for the agricultural policies of the Common Market, policies which have done little to help solve the age-old problems of the land. He approaches the farm problem from the point of view of the agricultural producers ignoring the price paid by consumers. He neglects the impact of high support prices for food on the wage scales outside agriculture. In Italy labor contracts include sliding cost-of-living adjustments. Food prices weigh heavily in the Italian consumer-price index, pushing it up, pushing wage scales up, pushing inflation up.

I don't know where he stands on the theory of functional integration. I think he doesn't believe that economic collaboration must "spill over" into political unification, but I am not sure. The minority of true European federalists in Italy (Altiero Spinelli, Ernesto Rossi, etc.) were not functionalists and supported the economic approach to collaboration only because their efforts to achieve direct political unification of Europe failed. I believe that Willis exaggerates the commitment of Alcide De Gasperi and the top leadership of the Christian Democratic party to European political union after 1950. He can cite chapter and verse for his position, but I think he takes this evidence too seriously and too literally. This hunch is not merely a personal judgment; it has behind it comments of people like Ernesto Rossi and Ugo La Malfa. It is also based on Italian practice within the EEC. The Italians are the grand mediators, always searching for a solution between the conflicts of the other members. But they battle tenaciously when Italian national interests are at stake.

Willis accepts uncritically the concept of partial sovereignty, a concept most political scientists reject. He neglects the distinction between devolving certain functions upon regional agencies and losing sovereignty, which cannot be sliced up. Not only de Gaulle but even more recently Heath and Pompidou have made that distinction clear.

Willis probably overestimates the impact of the Common Market on the Italian economic boom. While he says that it is impossible to measure accurately the contribution of the EEC to the growth of the Italian economy, he indicates that he considers the contribution to have been "vital." I think most economists believe otherwise and would give more credit to other factors in explaining the rapid growth of the Italian economy in the 1960's. I fear that Willis is also somewhat taken in by the arguments promoting "European-size" companies made possible by "European-size" markets because of the benefits derived from economies of scale. Very few industries require continental-wide markets to operate efficiently. The creation of these mammoth firms is as likely to result in elephantiasis as in economies of scale. His optimism is likewise exaggerated when he thinks that the economic level of southern Italy can be brought up to the average of the rest of the country by 1980. Although he cites the distinguished economist Pasquale Saraceno for this forecast we have no evidence in Italy, or elsewhere for that matter, to back it up.

But these are differences of degree between a slight optimist and a slight pessimist. His book remains a fine contribution to its subject, especially since the Italian role in European integration has suffered scholarly neglect.

NORMAN KOGAN
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International Law and the Resources of the Sea. By Juraj Andrassy. (New York: Columbia University Press, 1970. Pp. 191. $7.50.)

I found Professor Andrassy's book intellectually stimulating. His systematic treatment of the problem of exploitation of submarine resources is timely in its attention to a pressing problem. It is well written and clearly organized. The author says that his purpose ". . . is to examine the legal problems that have already arisen and will continue to arise as technological progress permits men to explore and utilize hitherto inaccessible submarine areas." This he has done well.

The book is divided into three Parts. Part I is concerned with natural phenomena and technological factors. Maps and statistical data are used to illustrate and reinforce his assertions. Part II deals with the legal concepts of the continental shelf, the fall-off, adjacency, use of the *thalweg* principle for measurement of the main currents of water separating islands from land masses, legal precepts for waters adjacent to islands controlled by nations far removed from these islands, and the exploitation of resources of the nonterritorial seas. In Part III, Professor Andrassy makes an inquiry into protection of the rights of small states, degree of participation in undersea exploitation by landbound states, equalization of outer limits of continental shelf in various locations, and feasibility of the exploitation of continental shelf resources at maximum depth, and he attempts to offer solutions for these unresolved issues. A select multilingual bibliography and an index are included.

The author has subjected existing conventional bilateral and multilateral agreements, to critical analysis and has demonstrated their unworkability owing to the rapid technological advances being made. He especially points out the obsoleteness of the 200-meter depth limitation imposed by these conventions and offers some guidelines for all future delimitations.

The author also considers international implications of some larger questions that must be resolved immediately by the community of nations. For example, shall the wealth of the sea become *res communis*, or will the greed for the control and possession of the resources of the sea by the major powers initiate submarine economic imperialism? Will the major powers possessed of technological knowledge and capable of large capital investments accept a plan for the development of these undersea resources submitted to the UN by the U.S., USSR, Malta or Tanzania? He points out the inability of UN to evolve an acceptable plan.

As an alternative, the author proposes the establishment of a new international organization, unencumbered by the politics and technicalities of the United Nations organization, with unconditional membership open to all nations; this organization would administer and regulate legislation relating to the continental shelf, sea farming, underwater communications, transportation, energy production, weather forecasting, and the prevention of undersea military installations. In all these issues, Professor Andrassy points to the inadequacy of current legal controls. He pleads for unanimity of action and for flexible agreements that provide for periodic re-evaluation and the adjustments necessitated by technical progress. He advocates a licensing system controlled by the proposed organization, parceling off areas of exploitation to individual states based on a predetermined and universally acceptable set of precepts, and proclamation of these resources of the sea as a common heritage of mankind.

This book is a valuable contribution to the literature. Whether or not one agrees with the author's premises and conclusions, it should be read widely, for the final solutions of these issues could eventually determine the fate of all humanity.

STANLEY S. JADOS

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Voting in the Security Council. By Sydney D. Bailey. (Bloomington: Indiana University Press, 1969. Pp. 275. Price $10.00.)

In this first volume of a projected three-volume study of the Security Council, Bailey sets out to describe voting, 1946–1967, in the Security Council. The result is a short book—the text is only 103 pages long, while the appendices cover 117 pages—that can be most accurately described as a repertory or source book of Security Council voting.

Bailey provides a competent, but largely predictable, discussion of the origins of Security Council voting arrangements; of the sometimes parliamentarily intriguing question of the double veto; of the ins-and-outs of the use of the veto. He also writes, all too briefly, about the growing Security Council practice of adopting resolutions without resorting to formal votes, and he outlines the role of the Security Council in voting on elections and appointments.

Its brevity and the comprehensiveness of its appendices may recommend this book to the scholar who desires to scan quickly the formal voting procedures of the Security Council. On the other hand, it ignores what to this reviewer, at least, are most of the important questions concerning voting in the Security Council. Data presentation is primitive and analysis almost totally absent. No attempt is made in the book to identify voting coalitions or patterns in the Security Council.

The author's formalistic focus leads him to ignore completely any consideration of the relationship between voting procedure and the policy outputs of the Security Council. For example, to what extent has the Security Council's inability to come to grips, even in a preliminary way, with many of the major issues of the day been the result of its voting procedures? Have these procedures shaped the voting coalitions formed, and if so, in what ways? Bailey also ignores the large issue of how much the formal decision-making structure of the Security Council reflects the real decision-making structure. If Bailey is correct in his assessment that the Security Council will continue to increase in importance, one can but hope that future analysis of its operations and procedures will move beyond the level of formal description found in this book.

DAVID A. KAY

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Preventing the Spread of Nuclear Weapons. Pugwash Monograph I, Review of C. F. Barnaby. (New York: The Humanities Press, 1969. Pp. 374. $9.50.)

This collection of essays on nonproliferation is the result of the First Pugwash Symposium held in London in April, 1968. It contains a wealth of information and many valuable insights on the technical and political aspects of controlling the spread of the military atom.

The book continues to be as timely as it is useful to the student of arms control and to other social scientists interested in how international politics and nuclear technology are related. The investigation of these relationships forms a substantial part of the scholarly literature on deterrence. But simply put, it is how nuclear forces and political judgments interact in the pursuit of national goals that has been the principal focus of inquiry. The impact that nuclear developments on the threshold of commercial feasibility have on international stability is a more recent and less understood phenomenon.

The essays discussed here are decidedly a contribution to our understanding of how technology and politics feed on each other to create the conditions for proliferation. The authors appreciate the complex interrelationships between civilian nuclear technology and diplomacy, and they explain the dual nature of the atom, which makes its widespread use so ominous.

A nonproliferation treaty ratified by most countries of the world—including those now reluctant to accede—would not prevent the eventual formation of a base of nuclear technology that could dramatically shorten the time span between the political decision to acquire nuclear weapons and the actual opening of a military option. This is especially so with "threshold" powers like Japan, West Germany, India, Israel, Italy, and Brazil. These, and China and France, either do not intend to accede or are temporizing and have postponed their ratification. About 70 states that do not have nuclear weapons have ratified the NPT, but the safeguards agreements to verify compliance remain to be negotiated between signatories and the International Atomic Energy Agency.

I share the apprehensions of most authors about a multipolar nuclear world (especially one with additional national nuclear forces of varying technical capabilities and disparate strategic doctrines). But I question the assumption about the effectiveness of even a universal nonproliferation treaty with safeguards that sustains the analysis in most of the essays. (A noteworthy exception is Vital's essay.)

Political difficulties are posed by the requirement that all "threshold" powers accede, and technical problems that might result from the need to account adequately for the amount of fissionable materials existing at the end of the fuel cycle. Beyond these there remains the very real issue that the nonproliferation treaty, even if universal and safeguarded, will, ironically, accelerate the transfer of nuclear technology to countries without weapons and hence increase the potential for the spread of nuclear weapons. This is particularly likely since the treaty obligates member states advanced in nuclear technology to promote the development of the civilian sector of the nuclear economy of non-weapon states.

Some authors are aware of the crucial role technology transfer will play in the future spread of nuclear weapons. Editor Barnaby notes this in his excellent summary essays, and he analyzes the problem at length in his own paper on "The Role of Reactor Exporting Countries." For the next decade, he tells us, only about 15 countries will be capable of producing nuclear power reactors; the remaining 90 per cent will have to import nuclear technology. Barnaby makes specific policy suggestions about how to safeguard this transfer—suggestions that should be carefully considered by international decision makers who want to prevent proliferation.

Despite their importance, the consequences under the NPT of nuclear technology transfer from advanced to developing countries is not a central concern of this book. The focal point of discussion and analysis is, instead, safeguards. Safeguards, in my opinion, are a questionable deterrent to the spread of nuclear weapons—notwithstanding the considerable energies, talents, and time devoted to their creation and implementation. They would obviously inhibit any clandestine program. But where are the financial and organizational resources the IAEA would need to apply verification effectively to signatory states? And is it realistic to expect that countries bent upon acquiring a military nuclear capability would either accede to the Treaty; or having adhered, choose a clandestine program over outright abrogation (a right given by the treaty)? Ultimately, verification is "national" and self-imposed. This alternative view is not investigated sufficiently—although a nationally based adversary system of verification is utilized by the Limited Test Ban Treaty, the most important negotiated measure of arms control in nonproliferation.

Insufficient analysis of security considerations is perhaps the most troublesome lacuna in this otherwise very useful contribution to the arms control literature. Perceived threats to national security are clearly the most important generator of incentives to proliferate. India's antagonism to the treaty, Nehru's legacy notwithstanding, and Japan's hesitation about ratifying it are eloquent in this regard. These considerations are touched upon by Higinbotham and Pomerance in their paper on "Non-Proliferation and the Arms Race," but their treatment is both insufficient and polemical. Arms control analysis must seriously address strategic factors, or it will decrease the chances for effective arms control.

Advocacy of arms control must be informed by expert knowledge of both technical and political aspects. These essays contribute greatly to the layman's understanding of the complex technical and political issues involved in proliferation. With the exception of Vital's discussion of the problems connected with guarantees, however, the discussion of the technical factors is richer and more balanced than that of the political aspects. An explanation may be that of

25 chapters, only five were written by social scientists or international lawyers, although at least 13 deal primarily with international politics.

The technical issues, dealt with separately, include examinations of: the prospects for the spread of nuclear reactors (V. S. Emelyanov, USSR); the comparative economics of nuclear and conventional fuels (Th. Rosenquist, geochemist, Norway); civilian uses of nuclear explosives (D. R. Inglis, physicist, U.S.A.) the cheapness of centrifuge separation (J. Beckman, physicist, U.K.) the technical components of the International Agency's safeguards system (S. Nilsson, physicist, and E. Falkström, physicist, Sweden); and the development of diversion-proof breeder reactors (A. M. Weinberg, physicist, U.S.A.).

Another section on "Political Aspects" discusses the lessons to be drawn from the proliferation of nuclear weapons that has already taken place (W. Mendl, political scientist, U.K.); methods of furthering universal acceptance of the NPT (J. Delbrucke, international lawyer, F.G.R.); nonproliferation and the arms race (W. A. Higinbotham, physicist, and Jo Pomerance, U.S.A.); the role of reactor exporting countries (C. F. Barnaby, physicist, U.K.); the viability of the Soviet-American draft treaty (P. L. Olgaard, physicist, Denmark); and international legal aspects relating to the participation of the two Germanys in the NPT (H. Kröger, international lawyer, G.D.R.).

Another on "Safeguards and Guarantees" touches lightly on technical factors but is essentially a discussion of international considerations. It treats safeguards and arms control; the long-term prospects for safeguards (J. Prawitz, chemist, Sweden); the lessons of the past on verification and security guarantees (D. Carlton, historian, U.K.); the general problem of guarantees (D. Vital, political scientist, Israel); the inadequacy of control under Article III of the nonproliferation treaty (B. Collen, chemist, Sweden); and, the issue of safeguards with special reference to East Germany (H. Faulstich, physicist, G.D.R.).

C. F. Barnaby has written an introduction and a summary of the discussions of the original symposium which are among the best of the essays. Convenient and useful appendices round out the volume.

The format itself perpetuates the traditional separation between technical and political analyses. What a splendid, *but missed,* opportunity to surmount the provincialism of both cultures!

Ciro E. Zoppo
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Die Aussenpolitik der Bundesrepublik: Erfahrungen und Masstäbe. By Waldemar Besson. (Munich: Piper Verlag, 1970. Pp. 493. Approx. $8.00.)

Strategie des Gleichgewichts: Deutsche Friedenspolitik und die Weltmächte. By Helmut Schmidt. (Stuttgart-Degerloch: Seewald Verlag, 1969. Pp. 328. Approx. $5.65.)

In recent years West Germans have begun to write about their country's foreign policy with sufficient detachment from the ongoing partisan debate to merit the attention of the scientific community. The late Waldemar Besson, until his death earlier this year Professor of Political Science at the University of Konstanz, and Helmut Schmidt, presently Minister of Defense in Chancellor Brandt's Cabinet, have written books that are interesting applications of international relations theory to the search for policies. In both cases the theoretical perspective is systemic, i.e., oriented toward interstate relations, and "realistic," i.e., the concerns are freedom of action, national interest, and military security.

Besson has written an historical account of West German foreign policy since 1949, following to a large degree the path which Wolfram F. Hanrieder had cut before him (in *West German Foreign Policy, 1949–1963,* [Stanford: Stanford University Press, 1967]). The compatibility of West Germany's foreign policy with the international system is Besson's main theme. But he has elevated the concept "compatibility," which for Hanrieder was an analytical category, to the level of a normative principle. For example, he argues that Adenauer's policy of integration with the West in the early 1950's was not intended to serve any other national goal beyond the one openly professed. That West Germany acquired a higher international status as a result of that policy is seen as a sort of bonus for well-adjusted behavior in the international society. After 1955, in Besson's view, this changed: The international system—characterized by an evolving *modus vivendi* between the superpowers—required the Federal Republic to pursue its interests with more independence from the United States. Besson criticizes Adenauer for failing to do so. In contrast to this period he portrays the slow and painful steps toward *détente* with Eastern Europe under Foreign Minister Gerhard Schroeder between 1961 and 1966 as a more fundamental change than the evidence would allow. Besson tends to overestimate the freedom of action which the West German government had in the late 1950's.

In the introduction Besson states that he has set out to find the *raison d'état* of the Federal Republic. This position characterizes his whole book: The world view is Federal Republican, not German. Thus Besson stands in the tradition of Adenauer. His conclusions on the Federal Republic's role in the international system of the 1970's reflect the mood of a large segment within the *Christian Democratic Union* (*CDU*) of which Gerhard Schroeder is one exponent.

For the future Besson advocates the development of West Germany's external relations along the lines followed by the present government. In Besson's view the present international system requires the nation-states to pursue their interests independently and vigorously. His major criticism of West German foreign policy is that it has not been independent and vigorous enough.

In contrast to Besson's historical treatment of the topic, Helmut Schmidt has organized his book as a systematic study of policy options with regard to one goal: the solution of the inherently dangerous anomaly of Germany's partition. Again the guiding principle is the compatibility of policies with the international system that is, in Schmidt's view, the European balance of power that guarantees the peace. Within this framework Schmidt addresses the same questions as did Besson.

Schmidt begins with an analysis of the worldwide strategic balance between the United States and the Soviet Union since the war. He differentiates this top layer of the international system and the regional European subsystem. Like Besson he notes the growing independence of the lesser European nations. And he emphasizes that the Europeans are not likely very soon to sacrifice their newly found independence for a supranational community. Schmidt's description of the European subsystem is detailed. The central part of this section of the book is his analysis of the military balance and of the security problem which has dominated European politics since the war. Together with a survey of the different interests displayed by the members of the European subsystem (including the Soviet Union and the United States), the analysis of the security problem lies at the base of his conclusions on the possible options for West German foreign policy in the 1970's.

In Schmidt's view a reunited Germany would have a critical size and power unacceptable to its neighbors in the West as well as in the East. The regional balance of power would be upset and peace would be destabilized. Even West Germany alone should not assert itself too strongly, Schmidt warns. To offset Western European fears of a German preponderance is one reason why he favors Great Britain's entry into the Common Market. The only chance Schmidt sees for approaching the reunification of Germany and achieving at least a certain measure of togetherness for the Germans is through a change of the regional subsystem. He would seek to achieve this by: (1) further stabilizing the military balance (including, hopefully, mutual and balanced force reductions), and (2) further developing the emerging *rapprochement* with Eastern Europe.

Comparing the two books, one finds a marked difference in the general outlook of the authors despite their similarities of approach. Besson is resigned to the status quo. His policies do not serve any apparent purpose except to keep the Federal Republic in the game. Schmidt wishes to change the status quo, but at the same time he is more concerned than Besson with possible threats to the status quo and with the problem of maintaining its beneficial stability. *Strategie des Gleichgewichts* consists of relatively independent chapters. This leads to a certain degree of duplication. Otherwise it is well organized and written in precise language. If references were added, it might well serve as an advanced text. In comparison, *Die Aussenpolitik der Bundesrepublik* suffers from vagueness arising from a sometimes portentous style. It does present, however, a useful analysis of West German foreign policy since 1949, and it is especially interesting for its specific Federal Republican perspective. Both books should be read by everyone who is interested in the topic.

JOACHIM HÜTTER

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International Monetary Reform, 1964–69. By Stephen D. Cohen. (New York: Praeger, 1970. Pp. 222. $13.50.)

Stephen D. Cohen has produced a very readable analysis of the political aspects of the international monetary reform negotiations preceding creation of the Special Drawing Rights. Unlike most historical studies of the period, Cohen's has not been made obsolete by the 1971 international foreign exchange crisis. In fact, he has no illusions about the limitations of the existing arrangements for international liquidity. It is most refreshing to perceive that a certain political realism has been injected into the academic analysis of the monetary system.

The political analysis might have been sharpened had the author dealt with some of the legal problems so brilliantly elucidated in various publications by Joseph Gold, Legal Counsel of the International Monetary Fund. At the drafting stage, a series of political differences of opinion were resolved through skillful use of legal language. Further, a flashback to Bretton Woods (viz., Richard N. Gardner's *Sterling-Dollar Diplomacy*) would have put the study into a sharper historical perspective, as would some of the political analyses produced by the economists as by-products of their work. It is true only for some economists in some instances that:

> Unfettered by political considerations, academic and private economists devised detailed and workable proposals in a matter of weeks. But it took the financial diplomats more than four years to devise a viable and acceptable reserve asset to supplement gold and reserve currencies (p. xvii).

Bernstein, Kenen, Machlup, *et al.*, worked on many detailed compromise proposals over long periods to accommodate political objections. Besides, there is a certain overlap between academic experts and international monetary negotiators. Triffin's prophetic previews of the dollar crisis took the political problem into account, and it was his consideration of politics that made him sound like Cassandra.

In reality, many of the private economists act as consultants to the financial diplomats and the monetary diplomacy establishment includes many of the private economists. The contributions of academia are very visible in monetary negotiations.

The author's theory of the potential use of economic and political power in international monetary affairs is confirmed by the United States' present attitude of "benign neglect" of the balance of payments problem. The author properly recognizes the obligations of countries which have a balance of payments surplus, thus providing some fuller understanding of the subsequent events of 1971.

In matters of political theory, one must concur with Cohen that the Morgenthau approach to power politics has a direct application to monetary developments. He is correct in stating that de Gaulle's intervention into international monetary affairs marked the formal politicalization of international monetary reform talks. It elevated them to the political summit at the declaratory stage, but this reviewer thinks it had little practical influence at the negotiating stage, although—and this is not stated in the book—it also evoked a statement from President Johnson. This type of Presidential summitry was distinctly absent in the 1971 confrontation on the dollar situation, which was handled entirely at the level of finance ministers and central bankers.

Since publication, we have had the American trade challenge added to the money challenge by the introduction of the surtax. We have had the toughest confrontation between the U.S. and foreign money managers and GATT in history. The dollar is no longer convertible into gold. As a result, we are moving toward a new monetary order in which Special Drawing Rights, first thought of as a palliative, may become the cornerstone.

The squabbles on devaluation and revaluation among European powers are not just a power play; they are also based on a curious mix of economic needs and prejudices. Between Americans and Europeans there has been a widening communications gap. Furthermore, European central bankers cannot simultaneously act in concert vis-à-vis the U.S.—which would give them some degree of strength—and preserve national interests in resolving conflicts with each other. Within nations, the illusion persists that central bankers work in a functionalist type of framework, and many of them have not yet accepted the realities of the political and economic power play in international monetary affairs.

As in 1969, when the Deutschmark was allowed to float upward and was then revalued, it became perfectly clear in 1971 that *national* economic interests were most significant. Economic orthodoxy and political sensitivities are, of course, evident in the German attitude regarding international liquidity policy, but in the reviewer's opinion *internal* economic pressures are equally important. The resistance to unilateral adjustment of an undervalued Deutschmark in 1969 stemmed largely from the German farmers' fear of noncompetitive export prices; in 1971, it emanated primarily from the apprehensions affecting large-scale German industry and German bankers who were overextended in the export business and found themselves under heavy price pressure. In both cases the economic motivations had political repercussions; the Socialists, who in the 1971 Government were reluctant even to allow a modest upward floating of the Deutschmark, had taken a pro-revaluation attitude when they were in the coalition Government in 1969.

This strange dichotomy may have arisen because in 1969 the economy was in high boom, and workers were most uneasy about the cost of living, while in 1971 the specter of difficulties in the heavy industry sector was clearly visible and, perhaps, outweighed the fear of inflation.

In conclusion, one can say that Stephen Cohen's book as a work of political analysis of the period preceding international monetary reform 1964–69 deserves very high marks. It is also sound in its general economic interpretations and gives a lucid analysis of the Special Drawing Rights agreement. When Cohen wrote his book he did not underestimate the disunity among the Common Market partners—a disunity that came to the fore again during the hectic negotiations on monetary tactics in 1971. Although this is a specialized study, limited in scope and time, it is nevertheless a further contribution to the growing literature which reminds us that modern economics originated in a discipline defined as political economy. In that general area, the classic of recent years is Charles P. Kindleberger's *Power and Money,* subtitled "The Politics of International Economics and the Economics of International Politics." Although both books appeared in the same year, the reviewer assumes that Cohen's book went to press before Kindleberger's, for otherwise the latter would have found an honorable place in the bibliography.

HENRY SIMON BLOCH
Columbia University

The Fourth Dimension of Warfare. Edited by Michael Elliott-Bateman. (New York: Praeger, 1970. Pp. 181. $6.50.)

This is the first in a planned series on *The Fourth Dimension of Warfare.* This volume's subtitle points to its focus: "Intelligence, Subversion, Resistance," and herein lies a basic problem. Such terminology covers many analytically separable activities. And so this volume contains a variegated set of essays, originally delivered as lectures at the University of Manchester. They range from accounts of the Jewish resistance against Rome nearly two thousand years ago to analyses of modern guerrilla warfare. It is somewhat difficult to find the thread that holds these essays together.

But it would be a mistake to dismiss this volume as a hodge-podge of unrelated parts. First, because some of the chapters do offer insight into important aspects of contemporary world politics. And second, because there is a shortage of information on modern warfare's "fourth dimension." This dimension Elliott-Bateman defines as use of irregular forces trained and equipped in all kinds of modern techniques to disrupt enemy forces, morale, and communications by "subversion, intelligence and resistance." In other words the subject is secret warfare—in all its imaginable forms.

This volume amply demonstrates that secret warfare has had its theorists and practitioners from earliest times. But never until the years of Cold War have governments established enormous and presumably permanent secret institutions as instruments of coercion in pursuit of foreign policy objectives. The development of modern weapons technology and its attendant nuclear "balance of terror" between the superpowers means that use of secret warfare will probably grow as a foreign policy instrument. And we know all too little about this "fourth dimension." Thus the subject demands our attention.

Three-quarters of this small volume deal with the past, centering mainly on experiences in the Second World War. Although it offers an interesting new interpretation, the chapter on "The Zealots: the Ancient Jewish Resistance Against Rome," by biblical scholar S. G. F. Brandon offers no striking relevance to contemporary analysis. M. R. D. Foot in two chapters deals with "Special Operations." Mr. Foot is the author of a previously published excellent study of Special Operations Executive (SOE) —the British equivalent of the U.S. Office of Strategic Services—operations in France in World War II. His chapters here are generalized and chatty, but they present a useful overview of a subject matter about which little has been written. Similarly, the late Donald McLachlan, British editor and journalist, deals in two chapters with intelligence, which may be regarded as a common denominator for all secret operations; McLachlan's cogent observations derive largely from his wartime experience in British naval intelligence.

Sir Colin Gubbins, as a British military professional, had extensive experience with irregular warfare and directed major British secret military operations in World War II. His reminiscences about his wartime experiences are valuable because so little is known about such events. Another chapter is contributed by Henri Raymond, who lived the dangerous life in World War II of a leader in the French underground resistance movement. His experiences, wittily recounted, illustrate concretely the nature of subversive warfare in an occupied country during war.

Finally, Elliott-Bateman, lecturer in military studies at the University of Manchester, attempts in two chapters to deal systematically with irregular warfare, which he chooses to call "people's war." His efforts do shed some light on a complex subject. Yet one leaves his concluding two chapters with the feeling of having read parts of a rough draft of an uncompleted, larger work. In addition to these essays, the editor has included a transcript of the question-answer period that followed each lecture. These offer additional insight into the topics. Another volume is scheduled to follow, and this may explain the somewhat truncated character of this collection.

This book must be categorized somewhere between that fairly voluminous and very uneven literature that appeals to the "secret warfare" buff, and the scanty scholarly literature that attempts to deal in a systematic way with a relatively unexplored but crucially important element in contemporary international relations. This is a difficult subject matter for scholarly analysis, but perhaps this slender volume will stimulate others to explore what undoubtedly is one of the frontiers for social science research.

HARRY HOWE RANSOM
Vanderbilt University

From Trust to Terror: The Onset of the Cold War, 1945–1950. By Herbert Feis. (New York: W. W. Norton, 1970. Pp. xx, 428. $10.00.)

Mr. Feis's books, laid end to end, begin to look like an American diplomatic *Forsythe Saga*. Depending on his longevity, this tale could continue indefinitely, although his writing pace now is appreciably slower than the movement of events. This current installment, a sequel to his *Atomic Bomb and the End of World War II*, now gets us to 1950. Since he began the series, with the *China Tangle* (1953) and *Roosevelt, Churchill and Stalin* (1957) the number of competitors entering the field (especially the Cold War part of it) have multiplied considerably. Since much of recent literature on this is self-consciously revisionist, *From Trust to Terror* gives one a sense of *déja vu*—as though the book had actually existed, somehow, before Horowitz, Alperowitz, Kolko, and others came along. His could be called an unrevised, or even unrepentant, standard version of the early Cold War.

This volume recounts, in a compellingly low-keyed style, how American policy-makers were confronted by and responded to concrete problems and crises with Russia after Roosevelt's death, and how a steady accumulation of quarrels, divergences of interest, and troubles piled up like bales of straw on the camel's back of "trust" in U.S.-Soviet relations. To change the metaphor, rivulets of conflicts over many specific matters joined, became streams, and merged into a mighty Mississippi of disagreement. It is clear, from this reading, that the most significant original (1945–1948) conflict-points, like the German one, were rendered so intractable not so much because of conflicts of ideology as because of conflicts of perceived responsibility. So the American aim of fostering economic recovery in Germany chiefly arose from a concern that a Germany diseased and impoverished would menace the social order of Europe, or remain indefinitely an expensive American charity case, or both. From a Soviet perspective, the German economy (what then was left of it) was an object of booty, to be used to restore the war-damaged Russian economy. A chaotic Germany was useful to Soviet aspirations in Western Europe. The conflict of these two aims, wholly aside from ideology, led powerfully toward the first major confrontation of wills over Berlin. In the American policy-makers' view, it was an accumulation of such pressure points, and not some concerted, prearranged strategy, which accounted for the decision to resist in a test of wills.

One special quality of Feis's work, is his clear preference for narrative, rather than "analytic," history. This mode, reminiscent of Ranke's *"wie es wirklich geschehen war,"* now is out of favor among many historians seeking a more respectable niche in the social sciences. But one needs to be reminded, from time to time, of the great necessity of books such as these; the current rewriting of narrative history by cold-war revisionists needs such standards, as Feis's, to keep it informed of the *sequence* of events, and what those events actually were. Feis's own bias, or perhaps preference, for looking at these events from a Washington view, reminds us again of how little even now we are permitted to know of Soviet intentions and crosscurrents of purpose.

PAUL SEABURY
University of California, Berkeley

The New Age of American Foreign Policy. By Frederick H. Hartmann. (New York: Macmillan, 1970. 399 pages. $9.95.)

Frederick Hartmann is presently Alfred Thayer Mahan Professor of Maritime Strategy at the Naval War College. The "New Age" of which he writes is the age of Harry Truman, Dwight Eisenhower, John Kennedy, and Lyndon Johnson. This book's overall theme is

> that the United States, once it had dealt rationally and realistically with its early problems . . . benefited almost without effort from a world made secure for reasons that Americans largely did not understand. When we then encountered a period (1914–1939) of grave stresses and strains, we

made little effort to understand their true source and reacted largely by an attitude of avoidance, 'going it alone.' When Pearl Harbor nonetheless brought unwanted war to our very door, we determined to play a 'proper' part in the world. Yet our concept of what was 'proper' was deduced largely by the avoidance of (or opposite of) pre-Pearl Harbor attitudes or actions. . . . Today, it is increasingly clear that such a new approach contains many of the defects of the old. This time it attempts too much rather than too little. But, as before, it is characterized by two failures: (1) to ask the right questions in the right sequence, and (2) to make *distinctions* in the relative effects and values of what is done. The record indicates that we still badly need to evolve more precise methods of thinking that will avoid the simple formulas of anticommunism and the like. To achieve more effectiveness involves encouraging thinking in terms of national interests and devising an appropriate foreign policy strategy which will *limit* our commitments to what other nations will not in the nature of things have to do for us anyhow—if we let them (pp. vii–viii).

We do indeed "still badly need to evolve more precise methods of thinking" but the record of those analysts who believe that "to achieve more effectiveness involves encouraging thinking in terms of national interests" seems as effective as the foreign affairs record of America's "new age" presidents and their secretaries of state and advisors. And this is rather well revealed in Hartmann's analysis. For he asserts in his analysis of American governmental foreign policy making in the book's second section that we must "discover, if possible, why the tendency has existed to downplay or ignore consideration of the grand strategic alternative foreign policy choices confronting the United States" (p. 67). But when in his third section he analyzes American foreign policy in the "new age" years of active involvement since 1945, he finds surprisingly little fault.

He asserts in his fourth section, on the problems of foreign policy, that "the conventional American view of the world—that ideological belief indicates and delineates basic national interests—almost automatically results in a strategic conception of [bloc] containment" (p. 270). This strategy, he claims, worked relatively well in Europe but has not worked well in Asia. He therefore argues for "an alternative strategy . . . designed to isolate a major opponent by utilizing the counterbalancing interests of its potential major allies. It is an *opponent-isolating* or *limiting strategy*" (p. 279).

Hartmann is confident that such an approach will suffice to serve "'the national interests of America." But he offers no major recasting of those "national interests" that would not correspond relatively well to that being attempted by the "neorealists" now proliferating inside as well as outside the government. While the text calls frequently for novelty, the analysis is generally almost as traditional as the short appended bibliography. Part of the difficulty probably stems from the limitations incumbent upon one attempting to write a comprehensive textbook which is also argumentative enough to merit and elicit the attention of students and even, perhaps, scholars.

Hartmann is at his best in his first section, which suggests features of American experience at home and abroad that can be seen to explain the foreign policies the United States has developed through its history, and in his occasional calling for novel rigor in our analyses and an opening of the foreign policy debate in the country to consideration of "grand strategic alternative foreign policy choices." Unfortunately, his analysis of the development of American policy and of influences upon it is so understanding of the policy that evolved and so sympathetic to the broadest outlines of that policy, that he himself seems not yet to have escaped the confines of the assumptions from which it is derived. Consequently, his historical understanding will be congenial to those sharing these assumptions, and almost unrecognizable to contemporary revisionists. And his analysis of contemporary policy problems and consequent conclusions will be pleasing to the Nixon administration.

It may be, as Hartmann clearly believes, that his analysis is about as venturesome as one can defensibly get. But his focus upon national interest as the organizing principle or cutting edge of his analysis may actually prevent him from seeing "the grand strategic foreign policy choices" that may even now be available. If he were to concentrate instead upon the patterns of interaction among states as the constituents and even more the constructors of the international "reality" that he believes so severely limits the effective choices of even the imaginative, he might find opportunities for a more significant and promising reconstruction of international politics. Were such to be the approach of American foreign policy makers, there might indeed develop a new age of American foreign policy. But that new age is not yet upon us, and the book that recommends the reconstruction has not yet appeared.

DAVID V. EDWARDS
University of Texas at Austin

The Alliance that Lost Its Way: A Critical Report on the Alliance for Progress. By Jerome Levinson and Juan de Onís. A Twentieth Century Fund Study. (Chicago: Quadrangle Books, 1970. Pp. 381. $7.95.)

In this skillfully written and well-reasoned volume, Jerome Levinson, a former official in the United States Agency for International Development, and Juan de Onís, a *New York Times* correspondent in Latin America, scrutinize the cornerstone of United States policy in Latin America for the past decade—the Alliance for Progress. Their judgment is stern, yet judicious. What was conceived as a program of economic development, democratic reform, and social change has produced instead "shattered hopes," "discord," and "disillusionment."

After a brief historical treatment of the emergence of the Alliance concept and program, Levinson and Onís trace the gradual metamorphosis of the Alliance from the euphoric idealism of the New Frontier to the pragmatic, economically oriented policy of the Johnson administration, and to its ultimate stagnation in 1968. The excessively ambitious goals set forth by the Kennedy Administration made disillusionment almost inevitable. And the sixteen military coups occurring in Latin America between 1961 and 1968, coupled with the waning fear of Castroism, created in the United States a growing disenchantment and indifference toward the Alliance.

But, more important, the Alliance in operation ran head-on against the traditional United States policy of safeguarding its economic and security interests at all costs. Despite lofty rhetoric about a peaceful revolution (a premeditated alternative to the Cuban model), the United States consistently subordinated social transformation and reform to the protection of United States corporate interests, to the promotion of financial and monetary stability, and to the preservation of United States hegemony in the Western Hemisphere.

As the authors illustrate in a series of excellent examples, the United States repeatedly violated the spirit of the Alliance. In Peru the Agency for International Development penalized the Belaúnde Terry government for its dispute with the International Petroleum Company, a subsidiary of Standard Oil of New Jersey, by restricting loan authorizations to $74.5 million between 1962 and 1968, as compared to $500 million for Chile and $450 million for Colombia. And in Brazil, an authorized fertilizer purchase was finally abandoned because it was impossible to comply with the requirement in the 1962 Foreign Assistance Act that at least fifty per cent of commodity tonnage financed by loan funds must be carried on United States vessels. The Alliance for Progress was merely one contender for favor among a host of competing business and bureaucratic forces.

After dissecting the failures of the Alliance, Levinson and Onís provide a valuable analysis of the unresolved problems demanding action in Latin America. Chapters 11 through 14 competently review the issues of agrarian reform, the population explosion, the urban crisis, and educational expansion. The responsibility of the United States to assist in the solution of these problems is implicit in the discussion.

But can the Alliance ideal be salvaged? Levinson and Onís think so. By avoiding excessive idealism and recognizing its own limitations, the United States can assist Latin American nations "determined to undertake economic and social reforms while strengthening the democratic process." As the authors admit, the United States cannot dictate policy, but their blueprint for promoting reform lacks clarity. One wonders whether the criteria they offer for allocating assistance to Latin American governments—"an effective political leadership, a reasonable economic management capability, and a sufficient consensus on objectives"—provide a workable solution. Perhaps the limitations on United States policy are even greater than the authors are willing to concede in their conclusion. The earlier acknowledgment by the authors that Cuba has come closer to some of the objectives of the Alliance than most Alliance members carries implications never fully explored.

Despite some weakness in the prescriptive aspect of their study, Levinson and Onís have written an outstanding critique of the Alliance for Progress and have provided a perceptive summary of the vital problems which the Latin American nations face internally and which the United States confronts in its foreign policy.

MICHAEL G. WEBSTER

Trinity University

International Community Power Structures: Comparative Studies of Four World Cities. By Delbert C. Miller. (Bloomington: Indiana University Press, 1970. Pp. 320, $11.50.)

Delbert C. Miller is among the earliest students of community power structure (circa Floyd Hunter) in its contemporary sense. Unlike most of the others, he is still publishing in this field. The book shows vividly the imprint of Miller's having grown with the field, and of his familiarity with its literature. Its main contribution lies in its pioneer effort to produce cross-national research with a degree of comparability—and in demonstrating the pitfalls in the process.

The concept of power is all-encompassing, "the network of influences that bear on all decisions that have general effect upon the community" (p. 273). The very breadth of this idea leads to many if not most of the ambiguities which crop up in the book, but I prefer here to accent strengths rather than weaknesses. A deceptively simple model of power structure ties together most of the research reported, though chapters 5 and 10 seem only tangentially related to the model. The model consists of the following five components: (1) institutional power structure of the (national) society, wherein each sector or institution (e.g., business, labor, religion *et al.*) has dominant values and resources which condition its power; (2) institutionalized power structure of the community, which reflects the pattern of the sectors of societal structure; (3) community power complex, consisting of associations and organizations having interests anchored in the sectors; (4) top influentials, who are individuals with influence proportional to that of the sectors; and (5) key influentials, who are top influentials of greatest power.

One observes immediately that the first element listed in the model is presented as the independent variable which conditions successively each of the others. Obviously Miller does not believe that all communities in the same society have identical structures, so he evidently is using the model as a purely heuristic device. Yet, going to some pains to rationalize and explain each variation from the model which his empirical data produce, he finds the model descriptive to a high degree. He is aided in this finding by the deliberate choice of study sites—four industrialized cities. Moreover, all four of the cities studied (Seattle, U.S.A.; Bristol, England; Cordoba, Argentina; and Lima, Peru) are regarded as examples of the same type of power structure. All are of the "cone" or "ring" type, depicted as a very thick, conical pie in which each of the aforementioned sectors is one of the slices. The perennial question arises, "Is the finding an artifact of the method?" Since the starting point is the listing of a number (variable from 9 to 13 for no reason this reviewer could discover) of sectors and an assessment of the relative power of each, it is hardly surprising that the power structure discovered consists of a cone. Had Miller studied Atlanta, could he have discovered the "stratified pyramidal structure" as Hunter did? Perhaps not. Moreover, is the cone really different from the polynucleated structures discovered by Dahl and by Sayre and Kaufman?

One of the most interesting statements in the book is, "that the model can approximate a predictable pattern, but that it will break down when community power is exercised by a collective representation that does not rely on highly personalized leadership" (p. 162). What Miller had found was that in the Latin American cities, religious and military sectors were very powerful but that these sectors did not produce key influentials. It is evident that he has put his finger on an important aspect of power which cannot be discerned through attribution of power to individuals. While Miller is puzzled by this—he says "other power mechanisms must be sought for explanation of sector influence" (p. 223)—we suspect the de-emphasis on issue-decision techniques may be the deficiency. Conceivably attribution of influence according to sectors (an innovation which Miller has introduced into power structure studies) is a technique which results in misperception on the part of the respondents, all of whom have less than total understanding of their own community, let alone of the norms, beliefs, and myths common to their cultures.

One further problem of sectorial attribution is that of definition of sectors. Business and finance constitute one sector while society and wealth represent another. How are these distinguished in the minds of respondents? Other sectors are government, political parties, education, and military. Are these not all "governmental"?

The above comments on the model now merge into comments on methodology. Hard-liners will be distressed that the four studies are not rigorously comparable as to method. I think, on the contrary, that in view of the state of the art Miller's methodological eclecticism is a strength. He employs virtually the whole range of methods for assessing power structures, including some innovations of his own. In a sense the book is focussed on the utilities of these various methods rather than on the findings they produce. The central core of comparability is attribution applied to individuals, community sectors, and societal sectors. Attribution is also used to identify the most important "positions" in the community, as well as the key issues recently in contest. The author compares with care the consequences of using attribution, position, and issue-decision approaches in identifying the holders of power. He finds all useful and necessary in filling out the picture, though at several points he shows partiality to attribution as the most valid method for identifying influentials.

The research design for each city varied, though the designs for Seattle and Bristol were similar, as were those for Cordoba and Lima. In part, this variation was introduced deliberately, on the ground that the most relevant research question or hypothesis varied according to the different circumstances of the target cities. In the Anglo-American cities the role of the business sector was given special attention, including clique behavior among businessmen. In the Latin American cities it was the institutional bases that provided hypotheses, together with the relationships between sectors. Among other innovations Miller ultimately arrived at a consideration of the role of value differences in decision-making (and presumably power structure). In a chapter which some might feel is not well integrated into the rest of the book, he reports research using fifteen scales (and proposing five additional ones for future use) measuring significant cultural differences among countries. He applies tests of validity and reliability to his satisfaction. While I do not completely understand how the measurement of these values is operationally relevant to power structure research, many of the problems of comparability that emerge in the book may well be related to these value differences. Miller may thus have opened the door to the solution of some of these problems.

There are many, many more matters raised by this study well worth comment, including its findings, which present few surprises. I have tried to convey the importance and flavor of the work, to my mind the most provocative book to date for any student about to enter the roiled waters of community power research.

ROBERT T. DALAND
University of North Carolina

Nuclear Diplomacy, the First Twenty-Five Years. By George H. Quester. (New York: the Dunellen Company, Inc., 1971. Pp. 327. $10.00.)

In this new book George Quester purports to give us a military history of the Cold War period, emphasizing the weapons availability and deployments that have determined or influenced international relations since 1945. Although I was at first interested in the effort, I soon found the work unsatisfactory. By attempting to be "nonpathological" through a self-imposed refusal to evaluate policies, emphasize mistakes, discuss available options, or suggest plausible reforms, Quester turns his book into an endless inventory of facts, generally well documented but without any clear focus, and therefore insufficiently selective and discriminate. When there is some analysis it is highly speculative: "the reasoning must be determined by inference," the readers are warned in the preface. Thus left seemingly free to select at will his explanations, the writer weaves a dialogue between himself and recent history, a dialogue in which the occasional interference by statesmen takes place under the strange assumption that they "all knew most of what they had to know and then decided to pretend ignorance wherever such a pretense was strategically appropriate" (p. xviii). In fact, given the nature of existing intelligence, one might regret that such ignorance was usually only feigned. The large number of Bisons and turboprop Bears which the Soviets were scheduled to produce by 1957 or the 200 Soviet ICBM's which were expected to be in place by 1960, strongly suggest that statesmen are indeed often as ignorant as they appear to be, not because of a lack of intelligence but possibly because of an excess of it.

The most unfortunate flaw in Quester's study is the lack of any unifying theme. Nuclear diplomacy is treated here in chronological sequences which usually cover one presidential term (1949, 1953, 1957, 1961, 1963, and 1969 are the cutting dates of the six chapters that comprise this book). Devoid of perspective, the author's account becomes a chronicle that narrates the whats of nuclear diplomacy but ignores the whys and the hows. For example, Quester finds the "strategic balance" which existed in the summer of 1949 "not such an unsatisfactory one . . . and brandishment of nuclear weapons might again keep the Russians deterred" (p. 57). But how is a strategic balance defined? And assuming that security (not defined either) is the ultimate goal of nuclear diplomacy, what can be said about the attainment (or lack of attainment) of such a goal? For that matter, what can be said about the manner in which the emergence of nuclear affluence has affected the perception of national security? Still more generally, in what ways does nuclear diplomacy differ from diplomacy in the pre-nuclear age?

Nor does the author devote much attention to the institutions and procedures of nuclear diplomacy. Occasionally, some statements may accidentally arouse the reader's attention to these questions, viz., "It may well be that officers of the particular services in fact come to believe the strategic arguments which bolster the role of their services, and that the distortion of the objective argument is therefore much more subtle" (p. 71). This looks like the beginning of an explanation of the recurring "gaps" which have emerged as basic fixtures of nuclear diplomacy. But the problem is not investigated further, and one is left with the unsatisfactory and gratuitous assumption that if tax dollars have often seemed wasted on useless weapons, and if such weapons have often brought the two superpowers dangerously close to war, "it may yet have been that each side clear-sightedly anticipated such harmful results as 'prisoners' in a game-theoretic prisoner's dilemma, and simply could do nothing about them" (p. xvii). But would this imply that the "prisoners" must remain trapped in their dilemma, and if not, how can they escape it?

Implicit in this chronicle is the author's endorsement of the idea that in matters of national defense the worst must always be assumed and prepared for. Thus, Quester approvingly notes that soon after the known failure of the Soviets to deploy many missiles by 1959–1960, "caution still required that the Russians perhaps be credited with a 300 to 80 ICBM superiority over the United States by 1962" (p. 193). But, as is often the case, such an approach is set aside when the distribution of strategic power was reversed and the possibility loomed that "the United States might have as many as 1,500 rocket vehicles capable of reaching the U.S.S.R. before the Russians obtained as many as 300 targetable against the United States" (p. 215). "The world," we are optimistically told, "in 1963 had learned to live with a strategic balance in which the United States could meaningfully threaten to strike first" (p. 283). But had the Russians learned as well? "In all fairness," Quester curtly acknowledges, "the Soviet Union may have every right to match the number of American missiles" (pp. 282–3). Thus, an American strategic buildup, motivated in part at least by the perception of a Soviet strategic buildup, led to a true Soviet strategic buildup. Here too one of Quester's introductory assumptions (that "trust and mistrust in either direction played a much smaller role than one might assume") does not appear to survive the evidence of his own narrative.

Despite the author's warning that his study would present nuclear diplomacy "as a series of deliberate engagements," this reviewer found that Quester's book does end by portraying the 1945–1968 Soviet-American arms race "as a string of aimless and blundering accidents." In summation, this book will give the readers a thorough, well-documented inventory of the arms race between the two superpowers. It will not, however, give them an improved understanding of that race, nor will it help them comprehend better the period during which that race was run.

SIMON H. SERFATY
University of California, Los Angeles

Russia and Nationalism in Central Asia: The Case of Tadzhikistan. By Teresa Rakowska-Harmstone. Published in cooperation with the Institute for Sino-Soviet Studies, the George Washington University. (Baltimore and London: The Johns Hopkins Press, 1970. Pp. xiii, 325. $10.95.)

Anyone who is interested in the subject matter of this book could do well by reading first the article, "What goes on in Soviet Asia?", in *Asian Affairs (Journal of the Royal Central Asian Society),* Feb. 1971, by Geoffrey Wheeler, who speaks highly of Professor Harmstone. There is a valid reason, in this case, for dragging into a book review an article by another author. We are dealing here with a delicately chilled Cold War topic. Colonel Wheeler marks out the larger framework within which Professor Harmstone carries out her detailed study. More than that, Colonel Wheeler is free of the vulgarities of American Cold Warriorism. He is an old-line British Military Intelligence man who believes that if the other side has a strong point, you should set it out in full, not try to smother it with Madison Avenue blah-blah. Perhaps one reason why Professor Harmstone is able to win his commendation is that, although she is published by one of the duller American university presses, she is able to work in the freer atmosphere of a Canadian university.

There are a number of currently fashionable theories about Soviet Asia:

(1) There is a Soviet imperialism which is merely a continuation of Tsarist imperialism, while the old capitalist imperialisms have shaded off into the recognition of new, independent nationalisms.

(2) Everything is being Russianized, including languages and cultural standards. The ideal of "the new Soviet man," set up for all the peoples of the Soviet Union, means in fact a Russianized man.

(3) These conditions cause a resentment which takes the form of anti-Russian nationalism, which year by year becomes more important than anti-communism. Sophisticated cold warriors have pretty well abandoned the "groaning in misery" description of populations living under Soviet rule, because it is evident that the material conditions of their life are better than they were in Tsarist times and, for the Central Asian republics, better than the conditions of their neighbors in Afghanistan and Iran.

Western experts assume that there will always be situations in which all Tadzhiks will rally together, as Tadzhiks, against Russians, or

Uzbeks, or somebody else. Soviet scholars call this "bourgeois" nationalism. All nationalisms, they argue, are dominated by a social class, and the Soviet task has been to shift the focus from the bourgeois class and feudal and capitalist heritages to the working class. Eventually, when all the nationalities of the multi-national Soviet Union are evenly led by an evenly developed working class, the "new Soviet man" will really come into being. My impression is, though I cannot cite authority, that when this has happened, individual survivals, like languages and cultural survivals that are not linked with class antagonisms, will still be acceptable. In the meantime Colonel Wheeler points out, on p. 52 of the article already cited, that the amount of publishing in languages other than Russian is growing, not diminishing.

For the handling of problems of this kind Professor Harmstone is better than the average—much better. She does not use the class terminology, but her presentation of a very wide, diversified range of phenomena is so clear that one can usually apply the litmus paper of class analysis for oneself, even if in an amateur way, and thus get some idea of how things that look red to non-Marxist eyes look blue to a Marxist; and this is absolutely necessary if coexistence is ever to supersede cold war.

Professor Harmstone vividly reconstructs the historical record. I hope my paraphrasing does not do her injustice. The point is that most of the facts are not disputed. It is the emphasis in presenting them that results in what some call balance, others distortion. When the revolution broke out, literacy among the Tadzhiks was low and was almost a monopoly of the old privileged classes. The Moslem clergy had long worked hand in hand with the wealthy and the landlords. It was easy to capture national feeling, turn it against the revolution, and delay the passage of power from the hands of the haves to those of the have-nots. The language barrier was highest between the Russian Bolsheviks and those whom theoretically they wanted to enlist and lead, the underprivileged classes. Moreover, many of them suffered from what may be called colonial disdain; the "ignorant natives" were to be commanded rather than led.

Later, as education improved, there arose the important question of whether, Tadzhik being a form of Persian, the new educated generation would look toward Iran (and the Uzbeks toward Turkey), rather than toward Russia. One can either say that a Tadzhik is compelled to learn Russian if he wants to get ahead in the professions, the bureaucracy, or the Party, or one can say that learning Russian is what enables him to enter a wider world. Turkey and Iran lag far behind the Soviet Union in modernization, science, and technology. For an Uzbek or a Tadzhik who wants to modernize himself at the highest contemporary world level, Turkish and Iranian are inferior to Russian.

OWEN LATTIMORE

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Nonaligned Black Africa: An International Subsystem. By George W. Shepherd, Jr. (Lexington, Mass.: D. C. Heath & Co. 1970. Pp. 151. $10.00.)

The breakup of the colonial empires has had profound effects on world politics. This is especially evident in Africa and Southeast Asia. In this study a specialist in African politics presents an analysis of the developments in Black Africa as they relate to international politics, with a concentration on the Black African nonalignment policy. The study is based on "a system or systemic approach."

The anti-colonialism and equalitarianism of African nationalism are the major sources of African nonalignment, but fear of great power domination and assimilation, the Cold War, fear of external subversion, and the quest for dignity and personality also play a part, according to Shepherd. Pan-Africanism, an ardent ideal of early African nationalism, has not been realized, but it gave birth to a nonaligned international subsystem that functions in spite of regional systems, and, asserts Shepherd, as long as the African subsystem can be maintained against internal and external pressures, the significance and prospect of nonalignment are made greater, but if the system is broken, certain African states might well align with great powers; even in this case, however, the policy of nonalignment might be continued by some of the regional systems. Shepherd holds that the strong African reaction against Rhodesia is producing a racial confrontation with very grave implications for the future of nonalignment.

So rapidly have events moved that a new force not mentioned in this study, namely, South Africa's outward policy is producing a reaction among African states which seriously threatens African unity and may lead the more radical governments to abandon nonalignment. About the only thing on which all African states are agreed is that the policy of *apartheid* is repulsive and abhorrent, but now there has developed disagreement on how to get rid of it. South Africa has offered Black African states nonaggression pacts and the willingness to engage in "dialogue" with them. Some seven states have indicated a willingness to enter into dialogue with Pretoria, presumably about its racial policy, though just what is to be discussed is not clear. President Felix Houphouet-Boigny of the Ivory Coast and Ghana's Premier K. A. Busia are two influential leaders who favor entering into dialogue with South Africa.

Shepherd emphasizes that a sharp difference of opinion exists between African and Western countries with respect to the priority to be given racial and colonial questions, the African countries regarding them as directly linked to keeping peace in the world. But now the African political leaders who are ready to talk with South Africa declare Communism to be a more serious threat than *apartheid,* which "neither divides the world nor threatens world peace." Other political leaders denounce this movement as dangerous and bound to divide Africa. Houphouet-Boigny does not wish to abandon neutrality or nonalignment, but he sees little possibility of getting rid of racialism in southern Africa by revolution, United Nations sanctions, or African Military action. Moreover, hostilities against South Africa would afford Communism an opportunity to intervene. He believes with President Banda of Malawi that dialogue with Pretoria would work to undermine *apartheid.*

Some studies are too broad in scope; this one is too limited. The policy of nonalignment cannot be easily abstracted from the complexities of African and world politics. Things are made to appear simpler than they are. Statements with wide implications are made with no discussion or with inadequate discussion. Shepherd states that the United States must abandon old concepts, such as the primacy of Europe; its policy is wedded to the status quo. Maybe so, but is not Europe still primary for the United States, and the world? To be sure, the status quo is not sacrosanct, but it must be the point of departure, and changes should not be made unless they offer hope for the better. Change is not an end in itself. These isolated sweeping statements are not always clear or consistent, as for example those on regional economic integration.

This volume contains much keen analysis and many sharp insights. It gives evidence of a wide and by no means superficial knowledge of African politics. It is, however, characterized by a certain scrappiness. To the reviewer, a superannuated political scientist, the "systemic" approach was puzzling. If it served to give clarity he would have welcomed it, but he did not find it of much aid to this end.

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Fact-Finding in the Maintenance of International Peace. By William I. Shore, Preface by A. J. P. Tammes. (Dobbs Ferry, New York: Oceana Publications, Inc., 1970. Pp. ii, 144, appendices and index. $15.00.)

Intelligence—assembling data about past and probable future events and investigating policy alternatives—is a decision function in its own right as well as a component of every other decision function. Thus, the promotion, prescription and termination of authoritative policy require a flow of reliable intelligence no less than do the invocation, application, and implementation of decisions. Appraisal of the aggregate performance of a decision process cannot be imagined without a rich flow of intelligence. The importance of intelligence for decision making is reflected in the prominence which law has given to the procedural and substantive criteria for the performance of this activity. Yet many of the dysfunctional features of formal law are also prominent in the intelligence function. Clearly, the improvement of decision, on the national as well as the international level, cannot be imagined without vastly improved intelligence procedures.

Dr. William Shore, in his *Fact-Finding in the Maintenance of International Peace,* has written an historical as well as analytical study. Fact-finding, as an institutionalized international activity, developed as an extremely attenuated and contingent form of intelligence gathering. It acquired its decisive form in the 1899 and 1907 Hague Peace Conferences, stimulated in part by the naive notions that there are "facts," that facts can resolve conflicts and that facts made known to the public will deter grosser deviations from normative standards. The Hague fact-finding model was influenced by the contemporaneous conception of consensual arbitration, but resulted in a report of the facts rather than a binding award based on facts and law. "Facts" are not, of course, social events, but are conclusions, derivative legal characterizations which draw on and apply policy and can affect behavior. Hence it is no surprise that states reluctant to enter third-party decision schemes did not stampede to fact-finding. The Hague system was used seven times, six of them on trivial matters. Fact-finding under the League, the Locarno Treaties, the General Act, and the Geneva Protocol was equally meager.

In the Inter-American system, fact-finding has often been used as a stopgap to suspend violence. While some fact-finding in the United Nations has followed the classical model, a number of variations have developed. In certain procedures, more institutionalized patterns of fact-finding have served fairly stable processes, such as those for the protection of human rights. And there has been a growing interest in the use of fact-finding commissions to provide the Security Council and the General Assembly with the data necessary for resolutions. Most recently, formal if *ad hoc* fact-finding in the Guinea invasion led to a quick condemnation of Portugal.

Dr. Shore's monographic study of fact-finding in the maintenance of international peace thus provides us with a comprehensive survey of this institutionalized activity from its inception to the present. After considering the conventional bases and a number of institutional settings, Shore surveys almost every case in which fact-finding was resorted to in international law. Perhaps the most valuable sections of the book are the analytic discussions of the initiation and full operation of fact-finding, for here the author goes beyond description to consider and recommend alternative procedures which can increase the effectiveness of fact-finding in the future. Throughout this part of the book, the author effects a dispassionate acceptance of the political character of this activity without despairing of rendering fact-finding a useful tool for the maintenance of the peace. Shore's expectations of what fact-finding can accomplish in the future are modest, primarily because of the claims of state sovereignty. His tendency to accept at par value the inflated worth of sovereignty in an interdependent world is one to which this reviewer would object. Dr. Shore's monograph is a useful contribution to the international legal literature.

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Nuremberg and Vietnam: an American Tragedy. By Telford Taylor. (Chicago: Quadrangle Books [A New York Times Book], 1970. Pp. 207. $5.95, cloth; $1.95, paper.)

Professor Taylor of Columbia Law School was one of the top Nürnberg protagonists. From late 1946 until Spring 1949, as Brigadier-General and Chief of Counsel of the U.S. Nürnberg War Crimes Tribunals, he prosecuted almost ten times as many leaders of the Third Reich as were tried by the preceding International (Four-Power) Nürnberg Tribunal.

His analysis of the relevance of "Nürnberg" to Vietnam is tightly reasoned, wide-ranging, thoroughly expert and yet very personal and deeply disturbing, first of all to the author. Only on the last page does the centrality of his thesis become clear—that the U.N. Charter and Nürnberg were twins, both designed to oppose violence and foster cooperation: the "American tragedy" alluded to in the sub-title is the *false invocation* of the "anti-aggression spirit of Nürnberg and the U.N. Charter" to *justify* our unforgivable destruction of Vietnam. But Taylor reaches this conclusion tortuously.

He first discusses basic concepts: law of war; war crime; illegal superior order; difference between just and unjust war. He dispels the myth that punishment of war crimes was invented at or limited to Nürnberg, and the even more scurrilous notion that there is, or should be, no law of war. He makes bold asides, e.g., "The rights and wrongs of Hiroshima are debatable, but . . . Dresden and Nagasaki were war crimes . . ." (p. 143); and the Bay of Pigs and Anglo-French Suez fiascos were "largely due to the attackers' feelings of guilt" (p. 185).

In analyzing various aspects of U.S. intervention in Vietnam in the light of "Nürnberg," Taylor takes at first cautious and very restricted views. Among attenuating or exculpating circumstances, he claims that "individuals of the yellow race are hard for our soldiers to identify" (p. 152)! There is excessive emphasis on the alleged uncertainty or shadowiness of some aspects of the law of war, and excessive indulgence for "military necessity."

Taylor ridicules "benighted Pentagon publicity" that represented our air bombardment of North Vietnam as "harmless to human beings," but he also disagrees with General Gavin that it was "militarily unsound and morally wrong": "I can see no sufficient basis for war crimes charges based on the bombing of North Vietnam" (p. 142). Whether the U.S. intervention in Vietnam constitutes "aggression" or "crime against peace" is declared to be an appropriate but unanswerable question.

Some arguments, *because* formalistically impeccable, reveal the full calamity in which the country finds itself. Taylor reminds us that Congress—not the President!—is abstractly "authorized" by the Constitution to abrogate American treaty obligations, even those of the U.N. Charter against the international use or threat of force, and could "in a fit of frenzy" authorize the execution of all prisoners-of-war (p. 116). It would be equally correct to state that the President could empty all Federal penitentiaries or order the bombing of U.S. cities. To make (internationally illegal) *use* of the fact that the Constitution "of course is superior to treaties . . ." (p. 116) denies, as Taylor himself shows, the quintessence of Nürnberg. Yet, midway in the book, the draft resister is told that his allegation of the illegality of the U.S. intervention under the U.N. Charter and other treaties can neither be denied nor authoritatively established; in any case, Congress could overrule any of them, and the draftee has to fight even in an internationally illegal war.

But the reader must read on; these are not the *final* views of an increasingly tormented conservative who until 1965 supported U.S. intervention in Vietnam as "aggression-checking undertaking in the spirit of the U.N. Charter" (p. 206). What in a lesser book would have to be criticized as contradictions constitute here the phases of "the education of Telford Taylor." A Picasso knows human anatomy. Especially toward the end, the book rises to a different level of sternness. It refers with increasing indignation to the enormities of American policies in Vietnam; juxtaposes shocking parallels between Nazi massacres in the Ukraine and Greece, and U.S. mass killings in Vietnam; and refers to the "mere gook rule."

He rejects Dean Rusk's accusation of North Vietnam's aggression as definitive (p. 184). While previously (p. 116) despairing that the Supreme Court could ever "unravel the tangle of treaties, agreements and resolutions," he eventually shows that certain justifications of the war (such as contained in Presidential statements) are *prima facie* indefensible. One of Taylor's reasons for opposing a "deus ex machina" role for the Supreme Court by declaring the war illegal (under Nürnberg or otherwise) appears to be his demand for moral redemption: Since Congress has for years so deeply identified itself with the war, "the President's course is to be checked by the Congress and not by the Court." He pointedly refers to impeachment.

Of great factual and doctrinal importance is Taylor's departure from strict separation between "crime against peace" and "war crime" by insisting that the war should not have been started because the decision-makers must have known that it could only be waged in the manner in which it has been. Whatever the original U.S. intentions, they "have by now been so completely submerged under the avalanche of death and destruction that they are no longer credible as descriptions of the operation as a whole" (p. 188).

Upon publication, the book received wide publicity mainly for two reasons: (1) Taylor's strong criticism of American military leadership in Vietnam since 1954, and his demand to hold that leadership responsible "from General Westmoreland down" (p. 181); and (2) his demand to extend the responsibility to commanders who *did not order*, but out of criminal negligence *failed to prevent*, atrocities by their subordinates. Two other of his conclusions deserve attention: (a) "given the course the war has taken and the depth and breadth of the opposition to its conduct" (p. 204), nobody must any longer be compelled to serve in it; and (b) "major responsibility for the war and the course it took . . . in the massive lethal dimensions [since 1964]" rests with "the Rusks, McNamaras, Bundys and Rostows" (p. 205).

Such a book should not be just grist for the mill. How can any impact be made on this country and its leadership? Soon after General Taylor's book made the news, it had to compete with the "battle hymn for Lt. Calley." Taylor's book also actually contains explanations of this phenomenon.

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The Imperial Order. By Robert G. Wesson. (Berkeley: University of California Press, 1967. Pp. 547. $10.00.)

Robert G. Wesson's *The Imperial Order* is a long overdue inquiry by an American political scientist into the rise and fall of empires. A study of this type would almost certainly have been undertaken a decade or two earlier except for the overweening concern of American political scientists with discrete, quantifiable studies cast in the mold of large-scale research in political behavior. Indeed, the supreme irony of American political science in the post-war era is that the fascination with political power, one of the few interests shared by both political behaviorists and political philosophers, has up to now seldom led to the study of the great experiments and adventures in the concentration of power which are the world empires. The price of this neglect is that deep and fundamental problems concerning the use and abuse of power have been largely ignored. Theories of social engineering and hypotheses about the organization and manipulation of power have crowded out discussions of the moral and intellectual perils of power. Yet for good rulers and bad alike, the use of power and its possible corrupting influence, which from Acton to Niebuhr has been a central issue, appears to have little place in the literature of contemporary political science.

The Imperial Order can be read at one or both of two distinct intellectual levels. It is first and foremost a saga about political empires, their rise and fall, manner of ordering power, underlying principles, intellectual and political failures, sources of strength and impoverishment, and link with universalism and Caesarism. In this venture, Wesson draws upon both original and secondary sources, paying heed to those studies which throw the greatest light on the problem of power.

At another level, the study is an inquiry in depth into the processes and routes by which conquest and aggrandizement have occurred, the links of empire with legitimacy myths, the benefits and burdens of highly concentrated power, and the life-cycle of the augmentation and disintegration of large-scale political societies. What distinguishes *The Imperial Order* from the philosophies of history of Spengler or Toynbee is Wesson's concern with essential problems of authority and power that historically if not recently have concerned political scientists. For their part, Spengler and Toynbee were caught up in trying to understand underlying social and religious forces that constituted great historical movements. For Wesson, the focus is more specifically on the organization, accumulation, and limitation of power as expressed in world empire. Past studies of empire have been primarily the work of historians and philosophers of history; Messon's contribution is to demonstrate that political scientists no less have perspective and insight in this area.

Another dimension of *The Imperial Order* is the author's continuing awareness that there are elements of imperialism and consequent strengths and failures and signs of greatness and decay in the American experience. Every second principle he adduces and nearly every moral he draws could be read in relationship to the American dream. For while the United States lacks the drives and motivations of empire which Wesson finds so essential, we have in fact been catapulted into an hegemonial position as a result of worldwide political and international forces. Moreover, it lies within our reach to avoid some of the worst abuses of power because to the extent that we are an empire, ours is a half-hearted imperialism, moderated and held in check by countervailing power within the society.

The principles that Wesson sets forth most explicitly about the great historical empires are not altogether new. He writes that "peripheral states have several advantages" (p. 8) in their relation to existing imperial centers. For one thing, they are often larger and freer to expand across open spaces loosely held by half-organized groups. Expansion at the vortex of international power is more fiercely resisted. Almost three decades ago, Toynbee wrote of a similar phenomenon—of external proletariats threatening existing imperial powers on their boundaries while themselves enjoying advantages of geography and relative freedom to expand. Wesson, with Toynbee, locates the will to empire in the psyche of a nation or people. He goes further in stating that "the most effective qualification for supreme power is the driving urge to clutch it—unqualified self-righteousness and determined will uneroded by civilized refinement" (p. 12). In this respect, the will to empire contains within itself seeds of disintegration; the empire runs the risk of losing its center and core from which came the original "driving urge." As Wesson says "In the widening of mental horizons, men look more beyond their own state or nation and think more of the general and universal . . . thereby weakening attachment to the native land" (p. 15).

Another topic of perennial concern is the relation between imperialism and morality. Wesson is little impressed with the prevailing theories of imperialism that go back to Marxist and post-Marxist thought or that reflect economic determinism. Instead, he finds that the drive toward empire has ineradicable political and psychological motivations:

> The nearer the goal of empire, the more alluring; and the assertion of expansive might brings confidence within and reputation for invincibility without. As the gap between supremacy and degradation narrows, the contest becomes one preeminently for power and only slightly for material interests. (pp. 18–19)

He sees little economic reason for the antagonism of commercial Athens and agricultural Sparta, or for that between Rome and Carthage, similarly land and sea powers. The Austro-German and Franco-Russian alliances before World War I were driven much more by fear than by hopes of gain. Neither Russia nor America has claims upon the possessions or vital economic interests of each other today, but it is a reality of world politics that no two powers can comfortably share world leadership. Imperialism, according to Wesson, is linked with the search for political security by men and nations who are doomed to feelings of insecurity but cling to the illusion that a few more increments of power will bring them to their goal. Markets and raw materials are far less the cause of imperialism than insecurity and "the more insecurity, the less room for morality" (p. 19).

For Wesson "The hunger for power is unlike that for food or fair women, and a conqueror can be happy only in ever more conquest" (p. 38). Here he writes in the tradition of political philosophers who have seen the unquenchable thirst for power as a condition of the ruler ending only in death. Imperialism is also subject to unbounded vanity and pride and for the powerful ruler it is an injury and an insult that men anywhere should defy his law. It is the habit of emperors to patronize the weak, hate and scorn the barbarians and in the end countenance only vassals. While ambitions and goals of empire are infinite, however, no historic universal empire has ever succeeded in conquering the world. Lesser powers have always remained who retained independence on the periphery. What is so remarkable in Wesson's study is that he finds this principle as relevant to the ancient Chinese empire over against the independence of Korea as to the Roman empire or the Old Kingdom of Egypt in relation to sturdy little states that resisted them on their borders and marches.

Specialized scholars, particularly in history and archeology, may well quarrel with Wesson's interpretations. Political scientists who seek discrete and researchable problems will take umbrage at the use he makes of historical data. Yet a bold and courageous intellectual endeavor of this sort, calling on the great mass of available historical and political literature, may help to redress the balance in a discipline torn from its moorings by the imperialism of those political scientists who have appropriated the whole of the discipline while practicing only a small and often insignificant part of it.

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Decision-Making for Space; Law and Politics in Air, Sea, and Outer Space. By Irvin L. White. (West Lafayette, Indiana: Purdue University Studies, 1970. Pp. 277. $6.50.)

In this doctoral dissertation, which received the 1968 best dissertation award from the Western Political Science Association, the author has attempted to perform a number of services. Taking the geographical areas of air, sea, and space he has examined a variety of factors which are thought to influence the decision making process in the international legal-political arena. Proceeding on the assumption that the product of the interaction of world legal and political forces, which emerges from the foregoing process, has entered the literature more as the "law-in-books" than as an empirical reflection of "law-in-action," he has sought to analyze selected problems so that efforts to resolve such problems can provide guidance as to world practice, present and future. Although acknowledging the existence of an international legal-political system, the author has limited his study to the behavior of national decision makers only.

Using an empirical approach, the author has identified the voting results at international air, sea, and space forums, such as special conferences or at the General Assembly of the UN. He has based important conclusions on "a modified majority rule principle." He urges that international law in action is the product of general and formal rather than universal acceptance. Thus, his methodology consists largely in examining the votes cast in such forums, identifying the number of signatories, and tabulating the entry into force of the relevant instruments. He then argues that such evidence supports the existence of universal legal-political obligations, at least where the majority of obligated States possesses a relevant special interest in the subject under consideration. After having examined the voting records, White also identifies the specific comments of many delegates to such international gatherings. Such observations are compared with the vote cast by the national representative. Quantitative methods and statistical analysis identify the relationship between votes cast and supportive dialogue. Whether such data constitute law in action or law in the books is not made clear. However, considering the vastness of the law of air, sea, and space it would have been an impossible task to examine the practice of international legal persons and relevant judicial holdings to measure more comprehensively the "in fact" or "in books" dichotomy.

The methodology also seeks to provide reasons why the national decision maker supports international law in the three areas of air, sea, and space. Assuming that there is merit in identifying a single predictor variable in the face of a wide variety of positions advanced by the conference delegates, White has attempted to determine which one variable will optimally predict whether a State will accept or reject (presumably both in words and in conduct) a particular political-legal rule or principle. The author used a total of thirty-six predictor variables. The reason for selecting this set is left unclear, other than that they had been used in other studies of international relations by other scholars.

The attempt to relate such variables to reality in this area of law and politics was unproductive, and White states that the desired level of explanatory significance and predictive utility had not been achieved, and, indeed, the variables were in a number of situations either misleading or totally uninterpretable. This is not to say, of course, that this research effort was wholly meaningless. The proof of the substantial irrelevance of the given predictor variables can lead either to the nonuse of this group of variables or to the positing of any new combination of new and old configurations. Through suitable trial and error this methodological form may some day have some use in international political-legal problem solving. Even supposing that the prodigious effort were to be made to determine whether any of the variable predictors used (including, for example, areal grouping, date of independence, former colonial ruler, constitutional status of current regime, miles of seacoast, security forces, etc.) possesses any true relevance; one might at least doubt whether such indicators would have as much meaning in identifying the present condition of formal political-legal commitments as do, for example, generally accepted principles of treaty interpretation.[1]

The author deserves credit for his innovative application of methodological procedures to so large an area of international law and politics. It is always immensely satisfying to examine the application of processes in new circumstances and surroundings. This effort may serve as a guide and as an inducement to others to consider these and other methodological approaches. The inadequacies reported in this study may be cautionary to later studies.

The book realistically demonstrates the dynamism which has called for new interpretations and imposed new meanings on the substantive principles and rules of air, sea, and space law following their formal identification in treaties and conventions. Much of the author's research and writing deals with pre-1965 events. Between 1965 and 1970, when the book was published, significant changes in attitudes have taken place respecting such formal treaties and conventions. Thus, many of his facts, law, assumptions, classifications, observations, and conclusions are somewhat lacking in current validity and sophistication. This does not detract from the historical data gathered in the book.

Regrettably, the publisher has marred the manuscript with perhaps more than the usual number of typographical errors. Some of them are misleading. Thus, the author, in discussing the fact that the space environment is reserved exclusively for peaceful purposes, unquestionably has sought to say that States have not "protested" against such peaceful uses, and the absence of such protests has been frequently mentioned in space environment legal writings. As printed, however, the passage reads "The widespread acceptance of this principle is emphasized by the failure of states to protect peaceful activities being conducted in space" (p. 198).

I join with the author in hoping that innovative studies of international affairs will lead to the emergence of processes for the better understanding of behavioral reality and that such reality will support such fundamental expectations as, for example, are contained in the current space treaty. Its principal expectation, of course, is that the space environment must be used exclusively for peaceful purposes.

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[1] Among such are literal, contextual, intent, implication, major purpose, practice, effectiveness, and survival considerations. When viewed from the perspective of practice there seems to be little doubt that policy influences provide guidance in the application of these principles.

INDEX TO VOLUME LXV

Abt, Clark C. *Serious Games.* Reviewed by Charles D. Elder. 1158

Abu-Lughod, Ibrahim, ed. *The Arab-Israeli Confrontation of June, 1967: An Arab Perspective.* Reviewed by Miles Copeland. 862

Adamany, David. *Financing Politics: Recent Wisconsin Elections.* Reviewed by Charles McCall. 516

Adams, John Clarke. Review of E. B. Fryde and Edward Miller, eds., *Historical Studies of the English Parliament.* 1208

The Administrative Process in Britain. R. G. S. Brown. Reviewed by Frederick T. Bent. 823

Administrative Reform. Gerald Caiden. Reviewed by Edwin A. Bock. 787

Alba, Victor. *The Latin Americans.* Book Note by Cole Blasier. 219

Albinski, Henry S. *Politics and Foreign Policy in Australia.* Book Note by Russell H. Barrett. 219

Algeria: The Politics of a Socialist Revolution. David and Marina Ottaway. Book Note by Brian Weinstein. 228

Alisky, Marvin. Book Note on John Womack, Jr., *Zapata and the Mexican Revolution.* 232

Allard, Sven. *Russia and the Austrian State Treaty: A Case Study of Soviet Policy in Europe.* Reviewed by William Welch. 863

Alliance Politics. Richard E. Neustadt. Reviewed by Francis E. Rourke. 880

The Alliance that Lost its Way: A Critical Report on the Alliance for Progress. Jerome Levinson and Jean de Onís. Reviewed by Michael G. Webster. 1250

Almond, Gabriel A. *Political Development.* Reviewed by Robert E. Dowse. 529

Altbach, Philip G., ed. *Turmoil and Transition: Higher Education and Student Politics in India.* Reviewed by Irene A. Gilbert 531

An Alternative Answer to: Who Pays for Defense? Jerry Hollenhorst and Gary Ault. 760

The American Enlisted Man. Charles C. Moskos, Jr. Reviewed by M. D. Feld. 803

American Foreign Policy in Europe: A Colloquium on Aspects of the American Presence in Belgium, The Federal Republic of Germany, Franace, Italy. Omer De Raeymaeker and Albert H. Bowman, eds. Reviewed by Donald Brandon. 871

The American Idea of Industrial Democracy, 1865–1965. Milton Derber. Reviewed by G. David Garson. 1175

American Images of Soviet Foreign Policy. William Welch. Reviewed by Robert G. Wesson. 1193

American Labor and United States Foreign Policy. Ronald Radosh. Reviewed by Robert W. Hattery. 813

American Political Parties: Social Change and Political Response. Everett Carll Ladd, Jr. Reviewed by William J. Crotty. 808

The American Secretaries of State and Their Diplomacy: Volume XVII, John Foster Dulles. Louis L. Gerson. Reviewed by Robert J. Bresler. 522

The Analysis of Political Cleavages. Douglas W. Rae and Michael Taylor. Reviewed by Kenneth A. Shepsle. 790

Anand, R. P. *Studies in International Adjudication.* Reviewed by Leo Gross. 865

An Anatomy of Values. Charles Fried. Reviewed by Ralph Ross. 193

Anderson, Charles W. *The Political Economy of Modern Spain: Policy-Making in an Authoritarian System.* Reviewed by Joan C. Ullman. 1196

Anderson, Charles W. Review of James W. Wilkie, *The Mexican Revolution: Federal Expenditure and Social Change Since 1910.* 572

Anderson, Stanley V., ed. *Ombudsmen for American Government?* Reviewed by Gerald R. McDaniel. 517

Anderson, Thornton. Review of David Lane, *The Roots of Russian Communism, A Social and Historical Study of Russian Social-Democracy 1898–1907.* 551

Andrassy, Juraj. *International Law and the Resources of the Sea.* Reviewed by Stanley S. Jados. 1242

Andrews, William G. Book Note on Frédéric Bon et al., *Le communisme en France et en Italie. Vol. I: Le communisme en France;* Annie Kriegel, *Les Communistes français;* and Waldeck Rochet, *L'Avenir du Parti Communiste français.* 221

Ankomah, Kofi. *Communication* on criticisms of his review of Thompson's book "Ghana's Foreign Policy: 1957–1966." 777

Antologia di Scienza Politica. Giovanni Sartori, ed. Reviewed by Giuseppe di Palma. 564

Appleton, Sheldon. Review of Jerome Alan Cohen, ed., *The Dynamics of China's Foreign Relations.* 582

Aquarone, Alberto. Review of Alan Cassels, *Mussolini's Early Diplomacy.* 869

The Arab-Israeli Confrontation of June, 1967: An Arab Perspective. Ibrahim Abu-Lughod, ed. Reviewed by Miles Copeland. 862

Armand, Louis and Michel Drancourt. *The European Challenge. A Detailed Blueprint on How Europe Can Use the Tools of the New Technology to Keep Pace with the 1970s.* Reviewed by Donald J. Puchala. 575

Ashcraft, Richard. Review of Jeremy Bentham (J. H. Burns and H. L. A. Hart, eds.), *An Introduction to the Principles of Morals and Legislation.* 784

Astiz, Carlos A. Review of F. LaMond Tullis, *Lord and Peasant in Peru: A Paradigm of Political and Social Change.* 855

Asymmetry in the Political System: Occasional Activists in the Republican and Democratic Parties, 1956–1964. David Nexon. 716

Atkinson, James D. *The Politics of Struggle: The Communist Front and Political Warfare.* Reviewed by Pi-chao Chen. . . . 576

The Atlantic Fantasy: The U.S., NATO, and Europe. David Calleo. Reviewed by Michael J. Brenner. 579

Ault, Gary and Jerry Hollenhorst. *An Alternative Answer to: Who Pays for Defense?* 760

Austin, Allan G. and Sherman Lewis. *Urban Government for Metropolitan Lima.* Reviewed by David Collier. 820

Avakumovic, Ivan. Review of W. J. Fishman, *The Insurrectionists.* 834

Avant, Gayle. Review of Economic Commission for Latin America, *Development Problems in Latin America.* 1203

Avnery, Uri. *Israel Without Zionists: A Plea for Peace in the Middle East.* Reviewed by Scott D. Johnston. 866

Bachrach, Peter. Review of Melvin Richter, ed., *Essays in Theory and History: An Approach to the Social Sciences.* 1168

Baerwald, Hans H. Review of Robert Guillain, *The Japanese Challenge;* and Herman Kahn, *The Emerging Japanese Superstate, Challenge and Response.* 1209

Bailey, Sydney D. *Voting in the Security Council.* Reviewed by David A. Kay. . . . 1243

Baker, Donald G. Review of Joel Kovel, *White Racism: A Psychohistory;* and Lawrence J. Friedman, *The White Savage: Racial Fantasies in the Postbellum South.* . . 1187

Balinky, Alexanader. *Marx's Economics, Origin and Development.* Reviewed by Murray Wolfson. 507

Bandwagon and Underdog Effects in Minimal-Information Elections. Daniel W. Fleitas. 434

Banfield, Edward C. and James Q. Wilson. *Political Ethos Revisited.* 1048

Banovetz, James M., ed. *Managing the Modern City.* Reviewed by Arthur Naftalin. . 1174

Barkun, Michael. Book Note on Jeremy Bentham, *Of Laws in General.* 202

Barnaby, C. F. *Preventing the Spread of Nuclear Weapons.* Reviewed by Ciro E. Zoppo. 1243

Barnes, Samuel H. Review of Giorgio Galli and Alfonso Prandi, *Patterns of Political Participation in Italy.* 835

Barrett, Russell H. Book Note on Henry S. Albinski, *Politics and Foreign Policy in Australia.* 219

Barton, Richard F. *A Primer on Simulation and Gaming.* Reviewed by Charles D. Elder. 1158

Baum, Richard. Review of Jerome Ch'en, ed., *Mao Papers: Anthology and Bibliography.* 1200

Bay, Christian. Review of Herbert J. Spiro, *Politics as the Master Science: From Plato to Mao.* 792

Beal, John Robinson. *Marshall in China.* Reviewed by O. Edmund Clubb. 532

Beer, Lawrence W. *Freedom of Information and the Evidentiary Use of Film in Japan: Law and Sociopolitics in an East Asian Democracy.* 1119

Behrman, Lucy C. *Muslim Brotherhoods and Politics in Senegal.* Reviewed by Donal Cruise O'Brien. 533

Behrman, Lucy C. Review of Donal Cruise O'Brien, *The Mourides of Senegal; The Political and Economic Organization of an Islamic Brotherhood.* 1202

Bendix, Reinhard. *Embattled Reason, Essays on Social Knowledge.* Reviewed by Scott Greer. 1159

Bent, Frederick T. Review of R. G. S. Brown, *The Administrative Process in Britain.* 823

Bentham, Jeremy (J. H. Burns and H. L. A. Hart, eds.). *An Introduction to the Principles of Morals and Legislation.* Reviewed by Richard Ashcraft. 784

Bentham, Jeremy. *Of Laws in General.* Book Note by Michael Barkun. 202

Berger, Morroe. *Islam in Egypt Today: Social and Political Aspects of Popular Religion.* Reviewed by Elmer N. Lear 534

Berger, Suzanne. Review of Lowell T. Noonan, *France: The Politics of Continuity in Change.* 848

The Berlin Police Force in the Weimar Republic. Hsi-huey Liang. Reviewed by Ronald F. Bunn. 1218

Bernays, Edward L. and Burnet Hershey, eds. *The Case for Reappraisal of U.S. Overseas Information Policies and Programs.* Reviewed by Eugene B. Mihaly. . . 577

Bernstein, Thomas P. Review of Joseph M. Kitagawa, ed., *Understanding Modern China.* 549

Besson, Waldemar. *Die Aussenpolitik der Bundeserpublik: Erfahrungen und Massstäbe.* Reviewed by Joachim Hütter. 1245

Between Philosophy and History: The Resurrection of Speculative Philosophy of

History Within the Analytic Tradition. Haskell Fain. Reviewed by Louis O. Mink. 1164

Bevans, Charles, compiled under the direction of. *Treaties and other International Agreements of the United States of America, 1776–1949,* Vols. I-IV. Book Note by Salo Engel. ... 234

Biafra: Selected Speeches and Random Thoughts of C. Ojukwu, with Diaries of Events. Odumegwu Ojukwu. Book Note by William John Hanna. ... 229

Big Business in Japanese Politics. Chitoshi Yanaga. Book Note by George O. Totten. 233

Birnbaum, Ervin. *The Politics of Compromise: State and Religion in Israel.* Reviewed by Norman L. Zucker. ... 821

Bisco, Ralph L., ed. *Data Bases, Computers and the Social Sciences.* Reviewed by Kenneth Janda. ... 785

Black, Antony J. Review of Joseph R. Strayer, *On the Medieval Origins of the Modern State.* ... 1171

Black, Cyril E. and Richard H. Falk, eds. *The Future of the International Legal Order, Vol. II: Wealth and Resources.* Reviewed by Ernst B. Haas. ... 584

Black, Cyril E. and Richard A. Falk, eds. *The Future of the International Legal Order, Vol. I. Trends and Patterns.* Book note by Thomas M. Franck. ... 237

Black, Cyril E. Review of Roy D. Laird, *The Soviet Paradigm: An Experiment in Creating a Monohierarchical Society.* ... 841

Blalock, Hubert M. *Theory Construction, From Verbal to Mathematical Formulations.* Reviewed by Charles F. Cnudde. .. 1161

Blanksten, George I. Reviews of Leo Huberman and Paul M. Sweezy, eds., *Régis Debrary and the Latin-American Revolution;* and J. Gregory Oswald and Anthony J. Strover, eds., *The Soviet Union and Latin America.* ... 837

Blasier, Cole. Book Note on Victor Alba, *The Latin Americans.* ... 219

Blitz, L. Franklin. Review of J. B. Webster and A. A. Boahen with H. O. Idowu. *History of West Africa.* ... 571

Bloch, Henry Simon. Review of Stephen D. Cohen, *International Monetary Reform, 1964–1969.* ... 1246

Blumberg, Paul. *Industrial Democracy: The Sociology of Participation.* Reviewed by G. David Garson. ... 1175

Boahen, A. A. and J. B. Webster with H. O. Idowu. *History of West Africa.* Reviewed by L. Franklin Blitz. ... 571

Bock, Edwin A., Review of Gerald Caiden, *Administrative Reform.* ... 787

Boichenko, Galina G. *The Political Organization of the USA: Social Institutions and their Interaction With the State.* Reviewed by Richard M. Mills. ... 795

Bon, Frédéric et al. *Le communisme en France et en Italie. Vol. I; Le communisme en France.* Book Note by William G. Andrews. ... 221

Bonilla, Frank and Myron Glazer. *Student Politics in Chile.* Reviewed by Ben G. Burnett. ... 822

Bosch, William J. *Judgment on Nuremberg.* Reviewed by Roberta S. Sigel. ... 796

Bourricaud, Francois. *Power and Society in Contemporary Peru.* Reviewed by Robert E. Scott. ... 536

Bowman, Albert H. and Omer De Raeymaeker, eds. *American Foreign Policy in Europe: A Colloquium on Aspects of the American Presence in Belgium, The Federal Republic of Germany, France, Italy.* Reviewed by Donald Brandon. ... 871

Brams, Steven J. aand Michael K. O'Leary. *Comment on Mayer's "A Note on 'An Axiomatic Model of Voting Bodies.'"* 766

Brandon, Donald. Review of Omer De Raeymaeker and Albert H. Bowman, eds., *American Foreign Policy in Europe: A Colloquium on Aspects of the American Presence in Belgium, The Federal Republic of Germany, France, Italy.* ... 871

Breckenridge, Adam Carlyle. *The Right to Privacy.* Book Note by Lois Moreland. ... 207

Brenner, Michael J. Review of David Calleo, *The Atlantic Fantasy: The U.S., NATO, and Europe.* ... 579

Bresler, Robert J. Review of Louis L. Gerson, *The American Secretaries of State and Their Diplomacy: Volume XVII, John Foster Dulles.* ... 522

British Politics and European Unity: Parties, Elites and Pressure Groups. Robert J. Lieber. Reviewed by D. J. Wenden. ... 1219

Bronfenbrenner, Martin. Review of Paul Mattick, *Marx and Keynes: The Limits of the Mixed Economy.* ... 789

Brooks, Hugh C. and Yassin El-Ayouty, eds. *Refugees South of the Sahara: An African Dilemma.* Reviewed by Channing B. Richardson. ... 537

Brown, Bernard E. Review of A. Belden Fields. *Student Politics in France: A Study of the Union Nationale des Etudiants de France.* ... 542

Brown, F. Gerald and Thomas P. Murphy, eds. *Emerging Patterns in Urban Administration.* Reviewed by Joseph M. Heikoff. 797

Brown, R. G. S. *The Administrative Process*

in Britain. Reviewed by Frederick T. Bent. 823
Brown, Robert. Review of Gwynn Nettler. *Explanations.* 1167
Brown, Robert E. *Carl Becker on History and the American Revolution.* Reviewed by Saul K. Padover. 518
Browning, Rufus P. Book Note on Donald Gerwin, *Budgeting Public Funds: The Decision Process in an Urban School District.* 211
Brunelli, Luciano et al. *La Presenza Sociale del PCI è della DC.* Reviewed by Alan J. Stern. 1197
Budgeting Public Funds: The Decision Process in an Urban School District. Donald Gerwin. Book note by Rufus P. Browning. 211
Bunn, Ronald F. Review of Hsi-huey Liang. *The Berlin Police Force in the Weimar Republic.* 1218
Burke, William T. *Towards a Better Use of the Ocean.* Reviewed by Wolfgang Friedmann. 578
Burma. F. S. V. Donnison. Reviewed by Josef Silverstein. 542
Burnett, Ben G. *Political Groups in Chile: The Dialogue Between Order and Change.* Reviewed by Francisco José Moreno. 1198
Burnett, Ben G. Review of Frank Bonilla and Myron Glazer, *Student Politics in Chile.* 822
Burnham, Walter Dean. *Communication* on Rusk's "The Effect of the Australian Ballot Reform on Split Ticket Voting: 1876–1908." 1149
Burns, J. H. and H. L. A. Hart, eds. Jeremy Bentham, *An Introduction to the Principles of Morals and Legislation.* Reviewed by Richard Ashcraft. 784
Burns, James MacGregor. Reviews of Jules Witcover, *The Resurrection of Richard Nixon;* and Garry Wills, *Nixon Agonistes: The Crisis of the Self-Made Man.* 818
Burns, James MacGregor. *Roosevelt: The Soldier of Freedom.* Reviewed by Harold F. Gosnell. 192
Butterworth, Robert Lyle. *Rejoinder to Riker's "Comment."* 747
Butterworth, Robert Lyle. *A Research Note on the Size of Winning Coalitions.* 741

Caiden, Gerald. *Administrative Reform.* Reviewed by Edwin A. Bock. 787
Caldwell, Lawrence T. Review of Thomas W. Wolfe, *Soviet Power and Europe, 1945–1970.* 884
Calleo, David. *The Atlantic Fantasy: The U.S., NATO, and Europe.* Reviewed by Michael J. Brenner. 579
Calvert, Peter. *Revolution.* Reviewed by H. Malcolm Macdonald. 825
Campbell, Blair. *Prescription and Description in Political Thought: The Case for Hobbes.* 376
Canton under Communism: Programs and Politics in a Provincial Capital, 1949–1968. Ezra Vogel. Reviewed by Chün-tu Hsüeh. 856
Canty, Donald. *A Single Society: Alternatives to Urban Apartheid.* Reviewed by Morris Levitt. 1176
Caporaso, James A. and Alan L. Pelowski. *Economic and Political Integration in Europe: A Quasi-Experimental Analysis.* 418
Caranfil, A. G. Review of Ian M. Matley, *Romania; A Profile.* 558
Carl Becker on History and the American Revolution. Robert E. Brown. Reviewed by Saul K. Padover. 518
Cartwright, John R. *Politics in Sierra Leone 1947–1967.* Reviewed by Clement Cottingham. 1199
Casanova, Pablo González. *Democracy in Mexico.* Book Note by Robert E. Scott. 222
The Case for Reappraisal of U.S. Overseas Information Policies and Programs. Edward L. Bernays and Burnet Hershey, eds. Reviewed by Eugene B. Mihaly. 577
Cassels, Alan. *Mussolini's Early Diplomacy.* Reviewed by Alberto Aquarone. 869
Casstevens, Thomas W. *Communication* on criticisms of his article "A Theorem about Voting" (March 1968). 187
Catholicism, Social Control, and Modernization in Latin America. Ivan Vallier. Reviewed by Kalman H. Silvert. 1221
Cattell, David T. Review of Robert Legvold, *Soviet Policy in West Africa.* 877
A Century of Russian Agriculture: From Alexander II to Khrushchev. Lazar Volin. Reviewed by Sidney I. Ploss. 1235
Chalupa, Dr. V. *Communication* on Wiatr's article "The Polish Party System." 777
Change in Communist Systems. Chalmers Johnson, ed. Reviewed by Melvin Croan. 547
The Changing Face of Communism in Eastern Europe. Peter A. Toma, ed. Reviewed by R. Barry Farrell. 1234
Changing Patterns of Local Government. A. F. Leemans. Reviewed by Francine F. Rabinovitz. 1212
Chapman, Samuel G. Review of Charles B. Saunders, Jr., *Upgrading the American Police.* 527
Cheit, Earl F. Review of John E. Coons, William H. Clune and Stephen D. Sugarman, *Private Wealth and Public Education.* 798

Ch'en, Jerome, ed. *Mao Papers: Anthology and Bibliography*. Reviewed by Richard Baum. 1200

Chen, Pi-chao. Review of James D. Atkinson, *The Politics of Struggle: The Communist Front and Political Warfare*. 576

Chittick, William O. *State Department, Press, and Pressure Groups: A Role Analysis*. Reviewed by John W. Outland. 1178

Christie, Ian R. *Myth and Reality in Late Eighteenth Century British Politics*. Review by J. G. A. Pocock. 826

Christol, Carl Q. Review of Irvin L. White, *Decision-Making for Space; Law and Politics in Air, Sea and Outer Space*. 1260

Chubb, Basil. *The Government and Politics of Ireland*. Reviewed by Frank J. Munger. 538

Cities of the Prairie. The Metropolitan Frontier and American Politics. Daniel J. Elazar. Reviewed by Richard H. Leach. 521

Citrin, Jack and Paul M. Sniderman. *Psychological Sources of Political Belief: Self-Esteem and Isolationist Attitudes*. 401

Clarke, James W. and E. Lester Levine.. *Marijuana Use, Social Discontent, and Political Alienation: A Study of High School Youth*. 120

Claude, Richard. *The Supreme Court and the Electoral Process*. Book Note by Peter G. Fish. 208

Clissold, Stephen, ed. *Soviet Relations With Latin America 1918–1968: A Documentary Survey*. Reviewed by Herbert S. Dinerstein. 581

Clubb, O. Edmund. Review of John Robinson Beal, *Marshall in China*. 532

Clune, William H., John E. Coons and Stephen D. Sugarman. *Private Wealth and Public Education*. Reviewed by Earl F. Cheit. 798

Cnudde, Charles F. Review of Hubert M. Blalock, *Theory Construction, From Verbal to Mathematical Formulations*. 1161

Cohen, Jerome Alan, ed. *The Dynamics of China's Foreign Relations*. Reviewed by Sheldon Appleton. 582

Cohen, Ronald. Review of Adam Kuper, *Kalahari Village Politics: An African Democracy*. 1216

Cohen, Stephen D. *International Monetary Reform, 1964–1969*. Reviewed by Henry Simon Bloch. 1246

Cole, George F. Review of Richard J. Richardson and Kenneth N. Vines, *The Politics of Federal Courts: Lower Courts in the United States*. 814

Collier, David. Review of Allan G. Austin and Sherman Lewis, *Urban Government for Metropolitan Lima*. 820

Collins, Edward, Jr., ed. *International Law in a Changing World: Cases, Documents and Readings*. Reviewed by Clifton E. Wilson. 583

Colonial Issues in British Politics, 1945–1961. David Goldsworthy. Reviewed by Trevor Lloyd. 1208

The Commander of the Faithful: The Moroccan Political Elite—A Study of Segmented Politics. John Waterbury. Reviewed by Clement Henry Moore. 570

Comment: Defense of Noncognitivism Defended. Felix E. Oppenheim. 1115

Comment on Butterworth, "A Research Note on the Size of Winning Coalitions." William H. Riker. 745

Comment: On Issues and Nonissues in the Study of Power. Frederick W. Frey. 1081

Comment on Mayer's "A Note on 'An Axiomatic Model of Voting Bodies.'" Steven J. Brams and Michael K. O'Leary. 766

Comment on Ordeshook, "Pareto Optimality in Electoral Competition." Martin Shubik. 1146

Communication by David Seidman on Hahn's "Correlates of Public Sentiment about War: Local Referenda on the Vietnam Issue." (December 1970). 1147

Communication by Gerald H. Kramer on Casstevens "A Theorem about Voting" (March 1968). 188

Communication by Harlan Hahn on criticism of his article "Correlates of Public Sentiment about War: Local Referenda on the Vietnam Issue." (December 1970). 1147

Communication by I. William Zartman on review of Thompson's *Ghana's Foreign Policy 1957–1966* (March 1970). 190

Communication by Jerrod Rusk on criticism of his "The Effect of the Australian Ballot Reform on Split Ticket Voting: 1876–1908." 1152

Communication by John P. Diggins on criticisms of his article "Ideology and Pragmatism: Philosophy or Passion" (September 1970). 190

Communication by Kol Ankomah on criticisms of his review of Thompson's book *"Ghana's Foreign Policy: 1957–1966"* (September 1970). 777

Communication by Lawrence S. Mayer in reply to communications by Kramer and Casstevens (March 1971). 779

Communication by Leon Hurwitz on Taylor and Herman "Party Systems and Government Stability." 1148

Communication by Michael Parenti on review of his book "The Anti-Communist Impulse." 780

Communication by Michael Taylor and V.

M. Herman on criticism of their "Party Systems and Government Stability." 1149

Communication by Stephen V. Stephens, on Aiken and Alford "Community Structure and Innovation: the Case of Public Housing." (September 1970). 499

Communication by Stephen W. Hartman on Diggins "Ideology and Pragmatism: Philosophy or Passion" (September 1970). .. 189

Communication by Thomas W. Casstevens on Criticisms of his article "A Theorem about Voting" (March 1968). 187

Communiccation by V. Chalupa on Wiatr's article "The Polish Party System." 777

Communication by Walter Dean Burnham on Rusk's "The Effect of the Australian Ballot Reform on Split Ticket Voting: 1876–1908." 1149

Comparative Federalism: The Territorial Dimension of Politics. Ivo D. Duchacek. Reviewed by S. Rufus Davis. 831

Comparative Politics and the Comparative Method. Arend Lijphart. 682

Congressional Oversight of Executive Agencies: A Study of the House Committee on Government Operations. Thomas A. Henderson. Reviewed by James W. Davis, Jr. 804

The Consolidated Treaty Series, 1648–1918. Clive Parry, LL.D., ed. Reviewed by Charles H. McLaughlin. 588

Contemporary Political Theory. Anthony de Crespigny and Alan Wertheimer, eds. Reviewed by Wilfrid E. Rumble. 1162

Coons, John E., William H. Clune and Stephen D. Sugarman. *Private Wealth and Public Education.* Reviewed by Earl F. Cheit. 798

Copeland, Miles. *The Game of Nations: the Amorality of Power Politics.* Reviewed by Michael Howard. 870

Copeland, Miles. Review of Ibrahim Abu-Lughod, ed., *The Arab-Israeli Confrontation of June, 1967: An Arab Perspective.* 862

Cottingham, Clement. Reviewed by John R. Cartwright, *Politics in Sierra Leone 1947–1967.* 1199

Cowan, L. Gray. Review of John Mensah Sarbah, *Fanti National Constitution, 2nd ed.* 852

Criddle, Byron. *Socialists and European Integration: A Study of the French Socialist Party.* Reviewed by Val R. Lorwin. 827

Crime in Urban Society. Barbara N. McLennan, ed. Reviewed by Robert C. Jacobs. 1189

Crisis in Brazil. Octavio Ianni. Reviewed by Ronald M. Schneider. 839

Croan, Melvin. Review of Chalmers Johnson, ed., *Change in Communist Systems.* 547

Cronin, Thomas E. Review of James W. Davis, Jr., *The National Executive Branch: An Introduction.* 799

Crotty, William J. *Party Effort and Its Impact on the Vote.* 439

Crotty, William J. Review of Everett Carll Ladd, Jr., *American Political Parties: Social Change and Political Response.* 808

Crow, John E. Reviews of Philip Mason, *Race Relations;* and John Rex, *Race Relations in Sociological Theory.* 845

Crow, John E. Review of Raymond Wolters, *Negroes and the Great Depression: The Problem of Economic Recovery.* 528

Crozier, Brian. *Since Stalin: An Assessment of Communist Power.* Reviewed by Myron Rush. 828

Custom and Government in the Lower Congo. Wyatt MacGaffey. Reviewed by M. Crawford Young. 557

Czechoslovakia: Intervention and Impact. I. William Zartman, ed. Reviewed by Edward Taborsky 573

Dahm, Bernhard. *Sukarno and the Struggle for Indonesian Independence.* Reviewed by Ann Ruth Willner. 540

Daland, Robert T. Review of Delbert C. Miller, *International Community Power Structures: Comparative Studies of Four World Cities.* 1251

Das Gupta, Jyotirindra. *Language Conflict and National Development: Group Politics and National Language Policy in India.* Reviewed by Baldev Raj Nayar. 830

Data Bases, Computers, and the Social Sciences. Ralph L. Bisco, ed. Reviewed by Kenneth Janda. 785

Data Processing: Applications to Political Research. Kenneth Janda. Reviewed by Charles Lewis Taylor. 1165

Dator, James Allen. Review of James W. White, *The Sōkagakkai and Mass Society.* 1240

Davis, J. Clarence III. *Politics of Pollution.* Reviewed by Ronald O. Loveridge. 1179

Davis, James W., Jr. *The National Executive Branch: An Introduction.* Reviewed by Thomas E. Cronin. 799

Davis, James W., Jr. Review of Thomas A. Henderson, *Congressional Oversight of Executive Agencies: A Study of the House Committee on Government Operations.* 804

Davis, S. Rufus. Review of Ivo D. Duchacek, *Comparative Federalism: The Territorial Dimension of Politics.* 831

Decision-Making for Space; Law and Politics in Air, Sea and Outer Space. Irvin L. White. Reviewed by Carl Q. Christol 1260

de Crespigny, Anthony and Alan Wertheimer,

eds. *Contemporary Political Theory.* Reviewed by Wilfrid E. Rumble. 1162
deLespinasse, Paul F. Review of Robert M. O'Neil, *The Price of Dependency: Civil Liberties in the Welfare State.* 812
Democracy in a Revolutionary Era: The Political Order Today. Harvey Wheeler. Book Note by Currin V. Shields. 206
Democracy in Mexico. Pablo González Casanova. Book Note by Robert E. Scott. ... 222
The Democratic Citizen: Social Science and Democratic Theory in the Twentieth Century. Dennis F. Thompson. Reviewed by John D. Lewis. 1172
de Onís, Juan and Jerome Levinson. *The Alliance That Lost its Way: A Critical Report on the Alliance for Progress.* Reviewed by Michael G. Webster. 1250
De Raeymaeker, Omer and Albert H. Bowman, eds. *American Foreign Policy in Europe: A Colloquium on Aspects of the American Presence in Belgium, The Federal Republic of Germany, France, Italy.* Reviewed by Donald Brandon. 871
Derber, Milton, *The American Idea of Industrial Democracy, 1865–1965.* Reviewed by G. David Garson. 1175
Deterrence and Persuasion. Wolf Mendl. Reviewed by Stanley Hoffmann. 878
Deutsch, Karl W. *On Political Theory and Political Action.* 11
Deutscher, Isaac. *Russia, China and the West.* Reviewed by Donald S. Zagoria. 541
Development Administration in Asia. Edward W. Weidner, ed. Reviewed by John T. Dorsey. 1237
The Development of Policy Thinking in Adolescence. Richard M. Merelman. 1033
Development Problems in Latin America. Economic Commission for Latin America. Reviewed by Gayle Avant. 1203
Dexter, Lewis Anthony. *How Organizations Are Represented in Washington.* Reviewed by Abraham Holtzman. 520
Dexter, Lewis Anthony. *The Sociology and Politics of Congress.* Reviewed by Robert L. Peabody. 1182
Die Aussenpolitik der Bundesrepublik: Erfahrungen und Masstäbe. Waldemar Besson. Reviewed by Joachim Hütter. 1245
Die Parlamentarischen Regierungssysteme in Europa. Klaus von Beyme. Reviewed by Karl Loewenstein. 858
Diggins, John P. *Communication* on criticisms of his article "Ideology and Pragmatism: Philosophy or Passion" (September 1970). 190
Dinerstein, Herbert S. Review of Stephen Clissold, ed., *Soviet Relations With Latin America 1918–1968: A Documentary Survey.* 581
Dionisopoulos, P. Alan. *Rebellion, Racism and Representation: The Adam Clayton Powell Case and Its Antecedents.* Reviewed by Robert S. Getz. 800
Di Palma, Giuseppe. Review of Giovanni Sartori, ed., *Antologia di Scienza Politica.* 564
The Division of Political Labor Between Mothers and Fathers. M. Kent Jennings and Richard G. Niemi. 69
Doing and Deserving: Essays in the Theory of Responsibility. Joel Feinberg. Reviewed by Glenn Tinder. 788
Donnison, F. S. V. *Burma.* Reviewed by Josef Silverstein. 542
Donovan, James A. *Militarism, USA.* Book Note by Morris Janowitz. 210
Dorsey, John T. Review of Edward W. Weidner, ed., *Development Administration in Asia.* 1237
Dowse, Robert E. Review of Gabriel A. Almond, *Political Development.* 529
Drancourt, Michel and Louis Armand. *The European Challenge. A Detailed Blueprint on How Europe Can Use the Tools of the New Technology to Keep Pace with the 1970s.* Reviewed by Donald J. Puchala. .. 575
Draper, Theodore. *The Rediscovery of Black Nationalism.* Reviewed by Robert E. Martin. 1180
Duchacek, Ivo D. *Comparative Federalism: The Territorial Dimension of Politics.* Reviewed by S. Rufus Davis. 831
The Dynamics of China's Foreign Relations. Jerome Alan Cohen, ed. Reviewed by Sheldon Appleton. 582

Early Writings. Walter Lippmann. Reviewed by Anwar Syed. 512
East Central Europe: A Guide to Basic Publications. Paul L. Horecky. Reviewed by George Klein. 1211
Eastern Europe. Norman J. G. Pounds. Reviewed by Peter A. Toma. 562
Ecker-Racz, L. L. *The Politics and Economics of State-Local Finance.* Reviewed by Arnold J. Meltsner. 801
The Ecology of Peasant Communism in India. Donald S. Zagoria. 144
Economic and Political Integration in Europe: A Quasi-Experimental Analysis. James A. Caporaso and Alan L. Pelowski. 418
Economic Commission for Latin America.

Development Problems in Latin America. Reviewed by Gayle Avant. 1203

Edwards, David V. Review of Frederick H. Hartmann, *The New Age of American Foreign Policy*. 1249

El-Ayouty Yassin and Hugh C. Brooks, eds. *Refugees South of the Sahara: An African Dilemma*. Reviewed by Channing B. Richardson. 537

Elazar, Daniel J. *Cities of the Prairie. The Metropolitan Frontier and American Politics*. Reviewed by Richard H. Leach. . . 521

Elder, Charles D. Review of Clark C. Abt, *Serious Games*. 1158

Elder, Charles D. Review of Richard F. Barton, *A Primer on Simulation and Gaming*. 1158

The Electoral Impact of Congressional Role Call Voting. Robert S. Erikson. 1018

Elliott-Bateman, Michael. ed. *The Fourth Dimension of Warfare*. Reviewed by Harry Howe Ransom. 1248

Embattled Reason, Essays on Social Knowledge. Reinhard Bendix. Reviewed by Scott Greer. 1159

The Emerging Japanese Superstate, Challenge and Response. Herman Kahn. Reviewed by Hans H. Baerwald. 1209

Emerging Patterns in Urban Administration. F. Gerald Brown and Thomas P. Murphy, eds. Reviewed by Joseph M. Heikoff. . . . 797

An End to Political Science: The Caucus Papers. Marvin Surkin and Alan Wolfe, eds. Reviewed by Vernon Van Dyke. 793

Engel, Salo. Book Note on Charles Bevans, compiler, *Treaties and other International Agreements of the United States of America, 1776–1949*, Vols. I-IV. 234

Engel, Salo. Review of Andrew Wilson Green, *Political Integration by Jurisprudence: The Work of the Court of Justice of the European Communities in European Political Integration*. 873

Erikson, Robert S. *The Electoral Impact of Congressional Role Call Voting*. 1018

Essays in Theory and History: An Approach to the Social Sciences. Melvin Richter. Reviewed by Peter Bachrach. 1168

Ethiopia: The Modernization of Autocracy. Robert L. Hess. Reviewed by Donald N. Levine. 836

Ethnicity, Party and National Integration: An Indonesian Case Study. R. William Liddle. Reviewed by Daniel Lev. 555

Eulau, Heinz, *Micro-Macro Political Analysis*. Reviewed by Michael J. Shapiro. 508

The European Challenge. A Detailed Blueprint on How Europe Can Use the Tools of the New Technology to Keep Pace with the 1970s. Louis Armand and Michel Drancourt. Reviewed by Donald J. Puchala. 575

Everyman's Constitution: Historical Essays on the Fourteenth Amendment, the "Conspiracy Theory" and American Constitutionalism. Howard Jay Graham. Book Note by Stephen B. Wood. 214

Explanations. Gwynn Nettler. Reviewed by Robert Brown. 1167

The Explosion: Marxism and the French Upheaval. Henri Lefebvre. Reviewed by A. Belden Fields. 843

Fact-finding in the Maintenance of International Peace. William I. Shore. Reviewed by W. M. Reisman. 1256

Fain, Haskell. *Between Philosophy and History: The Resurrection of Speculative Philosophy of History Within the Analytic Tradition*. Reviewed by Louis O. Mink. . . 1164

Falk, Richard A. and Cyril E. Black, eds. *The Future of the International Legal Order, Vol. 1. Trends and Patterns*. Book Note by Thomas M. Franck. 237

Falk, Richard A. and Cyril E. Black, eds. *The Future of the International Legal Order, Vol. II: Wealth and Resources*. Reviewed by Ernst B. Haas. 584

Falk, Richard A., ed. *The Vietnam War and International Law, Volume 2*. Book Note by Kline R. Swygard. 236

Fanti National Constitution, 2nd ed. John Mensah Sarbah. Reviewed by L. Gray Cowan. 852

Farrell, R. Barry, ed. *Political Leadership in Eastern Europe and the Soviet Union*. Reviewed by Michael M. Milenkovitch. . . 833

Farrell, R. Barry. Review of Peter A. Toma, ed., *The Changing Face of Communism in Eastern Europe*. 1234

Farrelly, David. Review of Charles A. Miller, *The Supreme Court and the Uses of History*. 1190

The Federal Courts as a Political System. Sheldon Goldman and Thomas P. Jahnige. Reviewed by C. Herman Pritchett. 1183

Federalists in Dissent: Imagery and Ideology in Jeffersonian America. Linda K. Kerber, Reviewed by Gordon Lloyd. 524

Feierabend, Ivo K. Review of Ted Robert Gurr, *Why Men Rebel*. 194

Feinberg, Joel. *Doing and Deserving: Essays in the Theory of Responsibility*. Reviewed by Glenn Tinder. 788

Feis, Herbert. *From Trust to Terror: The*

Onset of the Cold War, 1945–1950. Reviewed by Paul Seabury. 1249

Feld, M. D. Reviews of Jack D. Foner, *The United States Soldier Between Two Wars;* and Charles C. Moskos, Jr., *The American Enlisted Man.* 803

Fernbach, Alfred. Book Note on Arthur Stein, *India and the Soviet Union:* The *Nehru Era.* 231

Fields, A. Belden. *Student Politics in France: A Study of the Union Nationale des Etudiants de France.* Reviewed by Bernard E. Brown. 542

Fields, A. Belden. Reviews of Henri Lefebvre, *The Explosion: Marxism and the French Upheaval;* and Daniel Singer, *Prelude to Revolution: France in May 1968.* 843

Fifty Years of Communism: Theory and Practice, 1917–1967. G. F. Hudson. Reviewed by Slava Lubomudrov. 838

Financing Politics: Recent Wisconsin Elections. David Adamany. Reviewed by Charles McCall. 516

Finlay, David J. Book Note on Roger Genoud, *Nationalism and Economic Development in Ghana.* 225

Fischer, George, ed. *Science and Ideology in the Soviet Union.* Reviewed by Eugene Rabinowitch. 1204

Fischer, George. Review of Roland Gaucher, *Opposition in the U.S.S.R., 1917–1967;* and Sven Steenberg, *Vlasov.* 543

Fischer, Joseph, ed. *The Social Sciences and the Comparative Study of Educational Systems.* Reviewed by Byron G. Massialas. .. 1206

Fish, Peter G. Book Note on Richard Claude. *The Supreme Court and the Electoral Process.* 208

Fisher, Margaret W. and Leo E. Rose. *The Politics of Nepal: Persistence and Change in an Asian Monarchy.* Reviewed by Merrill R. Goodall. 851

Fishman, W. J. *The Insurrectionists.* Reviewed by Ivan Avakumovic. 834

Fleitas, Daniel W. *Bandwagon and Underdog Effects in Minimal-Information Elections.* 434

Foner, Jack D. *The United States Soldier between Two Wars.* Reviewed by M. D. Feld. 803

Foreign Policies in South Asia. S. P. Varma and K. P. Misra, eds. Reviewed by Arthur Stein. 882

Four Finns: Political Profiles. Marvin Rintala. Reviewed by John H. Hodgson. 1228

The Fourth Dimension of Warfare. Michael Elliott-Bateman, ed. Reviewed by Harry Howe Ransom. 1248

France: The Politics of Continuity in Change. Lowell T. Noonan. Reviewed by Suzanne Berger. 848

Franck, Thomas M. Book Note on Richard A. Falk and Cyril E. Black, eds., *The Future of the International Legal Order, Volume I. Trends and Patterns.* 237

Frankel, Joseph. *National Interest.* Reviewed by Werner Levi. 587

Frasure, Robert C. and Allan Kornberg, *Policy Differences in British Parliamentary Parties.* 694

Freedom of Information and the Evidentiary Use of Film in Japan: Law and Sociopolitics in an East Asian Democracy. Lawrence W. Beer. 1119

French Politicians and Elections, 1951–1969. Philip M. Williams. Reviewed by Vincent E. McHale. 859

French Socialists in Search of a Role, 1956–1967. Harvey G. Simmons. Reviewed by Vincent E. McHale. 199

Freund, Eric C. Review of Daniel P. Moynihan, ed., *Toward a National Urban Policy.* 810

Frey, Frederick W. *Comment: On Issues and Nonissues in the Study of Power.* 1081

Fried, Charles. *An Anatomy of Values.* Reviewed by Ralph Ross. 193

Fried, John H. E. Review of Albert Speer, *Inside the Third Reich.* 568

Fried, John H. E. Review of Telford Taylor, *Nuremberg and Vietnam: an American Tragedy.* 1257

Friedman, Lawrence J. *The White Savage: Racial Fantasies in the Postbellum South.* Reviewed by Donald G. Baker. 1187

Friedmann, Wolfgang. Review of William T. Burke, *Towards a Better Use of the Ocean.* 578

From Trust to Terror: The Onset of the Cold War, 1945–1950. Herbert Feis. Reviewed by Paul Seabury. 1249

Fryde, E. B. and Edward Miller, eds. *Historical Studies of the English Parliament.* Reviewed by John Clarke Adams. 1208

The Future of the International Legal Order, Volume 1. Trends and Patterns. Richard A. Falk and Cyril E. Black, eds. Book Note by Thomas M. Franck. 237

The Future of the International Legal Order, Vol. II: Wealth and Resources. Richard A. Falk and Cyril E. Black, eds. Reviewed by Ernst B. Haas. 584

Galli, Giorgio and Alfonso Prandi. *Patterns of Political Participation in Italy.* Reviewed by Samuel H. Barnes. 835

The Game of Nations: The Amorality of Power Politics. Miles Copeland. Reviewed by Michael Howard. 870

Garson, G. David. Reviews of Paul Blumberg, *Industrial Democracy: The Sociology of Participation;* and Milton Derber, *The American Idea of Industrial Democracy, 1865–1965.* 1175
Gaucher, Roland. *Opposition in the U.S.S.R., 1917–1967.* Reviewed by George Fischer. 543
Genoud, Roger. *Nationalism and Economic Development in Ghana.* Book Note by David J. Finlay. 225
Georges Mandel and the Third Republic. John M. Sherwood. Reviewed by Harvey G. Simmons. 565
Gerson, Louis L. *The American Secretaries of State and Their Diplomacy: Volume XVII, John Foster Dulles.* Reviewed by Robert J. Bresler. 522
Gerwin, Donald. *Budgeting Public Funds: The Decision Process in an Urban School District.* Book Note by Rufus P. Browning. 211
Getz, Robert S. Review of P. Alan Dionisopoulos, *Rebellion, Racism and Representation: The Adam Clayton Powell Case and Its Antecedents.* 800
Gilbert, Irene A. Review of Philip G. Altbach, ed., *Turmoil and Transition: Higher Education and Student Politics in India.* 531
Glazer, Myron and Frank Bonilla. *Student Politics in Chile.* Reviewed by Ben G. Burnett. 822
Goldman, Sheldon and Thomas P. Jahnige. *The Federal Courts as a Political System.* Reviewed by C. Herman Pritchett. 1183
Goldsworthy, David. *Colonial Issues in British Politics 1945–1961.* Reviewed by Trevor Lloyd. 1208
Goodall, Merrill R. Review of Leo E. Rose and Margaret W. Fisher, *The Politics of Nepal: Persistence and Change in an Asian Monarchy* 851
Goodman, Allan E. Review of Joint Development Group, *The Postwar Development of the Republic of Vietnam: Policies and Programs.* 1213
Goodwin, George, Jr. *The Little Legislatures: Committees of Congress.* Book Note by John F. Manley. 212
Gosnell, Harold F. Review of James MacGregor Burns, *Roosevelt: The Soldier of Freedom.* 192
The Government and Politics of Ireland. Basil Chubb. Reviewed by Frank J. Munger. 538
The Governmental System in Southern Rhodesia. D. J. Murray. Reviewed by James R. Scarritt. 559
Graham, Howard Jay. *Everyman's Constitution: Historical Essays on the Fourteenth Amendment, the "Conspiracy Theory" and American Constitutionalism.* Book Note by Stephen B. Wood. 214
Gray, Jack, ed. *Modern China's Search for a Political Form.* Book Note by Derek J. Waller. 226
Green, Andrew Wilson. *Political Integration by Jurisprudence: The Work of the Court of Justice of the European Communities in European Political Integration.* Reviewed by Salo Engel. 873
Greer, Scott. Review of Reinhard Bendix, *Embattled Reason, Essays on Social Knowledge.* 1159
Gregg, Robert W. Review of C. Wilfred Jenks, *The World Beyond the Charter in Historical Perspective: A Tentative Synthesis of Four Stages of World Organization.* 874
Gross, Leo. Review of R. P. Anand, *Studies in International Adjudication.* 865
Grusky, Oscar and George A. Miller, eds. *The Sociology of Organizations: Basic Studies.* Book Note by Philip Kronenberg. 203
Guillain, Robert. *The Japanese Challenge.* Reviewed by Hans H. Baerwald. 1209
Gurr, Ted Robert. *Why Men Rebel.* Reviewed by Ivo K. Feierabend. 194

Haas, Ernst B. Review of Richard A. Falk and Cyril E. Black, eds., *The Future of the International Legal Order, Vol. II: Wealth and Resources.* 584
Hahn, Harlan, *Communication* on criticism of his article "Correlates of Public Sentiment about War: Local Referenda on the Vietnam Issue." (December 1970). 1147
Hamilton, Howard D. *The Municipal Voter: Voting and Nonvoting in City Elections.* 1135
Hanna, William John. Book Note on Odumegwu Ojukwu, *Biafra: Selected Speeches and Random Thoughts of C. Ojukwu, with Diaries of Events.* 229
Hanreider, Wolfram F. Review of Hans J. Morgenthau, *Truth and Power: Essays of a Decade, 1960–70.* 879
Harik, Iliya F. *Opinion Leaders and the Mass Media in Rural Egypt: A Reconsideration of the Two-Step Flow of Communications Hypothesis.* 731
Hart, H. L. A. and J. H. Burns, eds. Jeremy Bentham, *An Introduction to the Principles of Morals and Legislation.* Reviewed by Richard Ashcraft. 784
Hartmann, Frederick H. *The New Age of American Foreign Policy.* Reviewed by David V. Edwards. 1249

Hartman, Stephen W. *Communication* on Diggins "Ideology and Pragmatism: Philosophy or Passion" (September 1970). 189

Hattery, Robert W. Review of Ronald Radosh, *American Labor and United States Foreign Policy*. 813

Hayter, Sir William. *Russia and the World: A Study of Soviet Foreign Policy*. Book Note by Alvin Z. Rubinstein. 227

The Heathen Chinee, A Study of American Attitudes Toward China, 1890–1905. Robert McClellan. Reviewed by Harold R. Isaacs. 1189

Heikoff, Joseph M. Review of F. Gerald Brown and Thomas P. Murphy, eds., *Emerging Patterns in Urban Administration*. 797

Heinrich Himmler—A Nazi in the Making, 1900–1926. Bradley F. Smith. Reviewed by Raul Hilberg. 1230

Henderson, Thomas A. *Congressional Oversight of Executive Agencies: A Study of the House Committee on Government Operations*. Reviewed by James W. Davis, Jr. 804

Hennessey, Bernard C. *Political Internships: Theory, Practice, Evaluation*. Book Note by James T. Murphy. 215

Herman, V. M., and Michael Taylor, *Communication* on criticism of their "Party Systems and Government Stability." 1149

Herman, V. M. and Michael Taylor, *Party Systems and Government Stability*. 28

Hershey, Burnet and Edward L. Bernays, eds. *The Case for Reappraisal of U.S. Overseas Information Policies and Programs*. Reviewed by Eugene B. Mihaly. 577

Hess, Robert L. *Ethiopia: The Modernization of Autocracy*. Reviewed by Donald N. Levine. 836

Hilberg, Raul. Review of Bradley F. Smith, *Heinrich Himmler—A Nazi in the Making, 1900–1926*. 1230

The Himalaya Borderland. Ram Rahul. Reviewed by Kunjar M. Sharma. 1228

Hinckley, Barbara. Review of Stephen Horn. *Unused Power*. 1185

Hirszowicz, L. Review of George W. Stocking, *Middle East Oil: A Study in Political and Economic Controversy*. 881

Historical Studies of the English Parliament. E. B. Fryde and Edward Miller, eds. Reviewed by John Clarke Adams. 1208

The History of the Nazi Party: 1919–1933. Dietrich Orlow. Reviewed by Peter W. Nixdorff. 849

History of West Africa. J. B. Webster and A. A. Boahen with H. O. Idowu. Reviewed by L. Franklin Blitz. 571

Hobbes and the Science of Indirect Government. Harvey C. Mansfield, Jr. 97

Hodgson, John H. Review of Marvin Rintala. *Four Finns: Political Profiles*. 1228

Hoffmann, Stanley. Review of Wolf Mendl, *Deterrence and Persuasion*. 878

Hollenhorst, Jerry and Gary Ault, *An Alternative Answer to: Who Pays for Defense?* 760

Holt, Robert T. and John E. Turner, eds. *The Methodology of Comparative Research*. Reviewed by G. Bingham Powell, Jr. 545

Holtzman, Abraham. *Legislative Liaison: Executive Leadership in Congress*. Reviewed by Charles O. Jones. 805

Holtzman, Abraham. Review of Lewis Anthony Dexter, *How Organizations Are Represented in Washington*. 520

Horecky, Paul L. *East Central Europe: A Guide to Basic Publications*. Reviewed by George Klein. 1211

Horn, Stephen. *Unused Power*. Reviewed by Barbara Hinckley. 1185

Horwitz, Robert H. Review of Arnold A. Rogow, ed., *Politics, Personality and Social Science in the Twentieth Century: Essays in Honor of Harold D. Lasswell*. 1169

Howard, J. Woodford, Jr. *Judicial Biography and the Behavioral Persuasion*. 704

Howard, Michael. Review of Miles Copeland, *The Game of Nations: the Amorality of Power Politics*. 870

How Organizations Are Represented in Washington. Lewis Anthony Dexter. Reviewed by Abraham Holtzman. 520

Hsüeh, Chün-tu. Review of Ezra Vogel, *Canton under Communism: Programs and Politics in a Provincial Capital, 1949–1968*. 856

Hsüeh, Chün-tu. Review of John Wilson Lewis, ed., *Party Leadership and Revolutionary Power in China*. 553

Huberman, Leo and Paul M. Sweezy, eds. *Régis Debray and the Latin-American Revolution*. Reviewed by George I. Blanksten. 837

Hudson, G. F. *Fifty Years of Communism: Theory and Practice, 1917–1967*. Reviewed by Slava Lubomudrov. 838

Humes, Samuel and Eileen Martin. *The Structure of Local Government*. Reviewed by Francine F. Rabinovitz. 1212

The Hungarian Soviet Republic, 1919. Iván Völgyes. Reviewed by Bennett Kovrig. 857

Hurwitz, Leon, *Communication* on Taylor and Herman "Party Systems and Government Stability." 1148
Hütter, Joachim. Review of Waldemar Besson, *Die Aussenpolitik der Bundesrepublik: Erfahrungen und Masstäbe;* and Helmut Schmidt, *Strategie des Gleichgewichts: Deutsche Friedenspolitik und die Weltmächte.* 1245

Ianni, Octavio. *Crisis in Brazil.* Reviewed by Ronald M. Schneider. 839
Ideology and Inconsistency: The Cross-Pressured Nigerian Worker. Robert Melson. 161
Imaz, José Luis de. *Los Que Mandan.* Reviewed by Peter Ranis. 546
The Imperial Order. Robert G. Wesson. Reviewed by Kenneth W. Thompson. 1258
Ind Defense of Sovereignty. W. J. Stankiewicz, ed. Reviewed by Robert K. Woetzel. 590
India and Ceylon: Unity and Diversity. A Symposium. Philip Mason, ed. Reviewed by Richard L. Park. 845
India and the Soviet Union: The Nehru Era. Arthur Stein. Book Note by Alfred Fernbach. 231
Industrial Democracy: The Sociology of Participation. Paul Blumberg. Reviewed by G. David Garson. 1175
Inglehart, Ronald. *The Silent Revolution in Europe: Intergenerational Change in Post-Industrial Societies.* 991
Inside the Third Reich. Albert Speer. Reviewed by John H. E. Fried. 568
The Insurrectionists. W. J. Fishman. Reviewed by Ivan Avakumovic. 834
International Community Power Structures: Comparative Studies of Four World Cities. Delbert C. Miller. Reviewed by Robert T. Daland. 1251
International Law and the Resources of the Sea. Juraj Andrassy. Reviewed by Stanley S. Jados. 1242
International Law in a Changing World: Cases, Documents and Readings. Edward Collins, Jr., ed. Reviewed by Clifton E. Wilson. 583
International Law in an Organizing World. William L. Tung. Reviewed by Clifton E. Wilson. 583
International Monetary Reform, 1964–1969. Stephen D. Cohen. Reviewed by Henry Simon Bloch. 1246
An Introduction to the Principles of Morals and Legislation. Jeremy Bentham. (J. H. Burns and H. L. A. Hart, eds.) Reviewed by Richard Ashcraft. 784
Isaacs, Harold R. Review of Robert McClellan, *The Heathen Chinee, A Study of American Attitudes Toward China, 1890–1905.* 1189
Islam in Egypt Today: Social and Political Aspects of Popular Religion. Morroe Berger. Reviewed by Elmer N. Lear. ... 534
Israel Without Zionists: A Plea for Peace in the Middle East. Uri Avnery. Review of Scott D. Johnston. 866
Issue Salience and Party Choice. David E. RePass. 389
The Italians in Chicago 1880–1930: A Study in Ethnic Mobility. Humbert S. Nelli. Reviewed by Edgar Litt. 1191
Italy Chooses Europe. F. Roy Willis. Reviewed by Norman Kogan. 1241

Jackson, John E. *Statistical Models of Senate Roll Call Voting.* 451
Jacobs, Dan N. Review of Warren Lerner, *Karl Radek: The Last Internationalist.* .. 552
Jacobs, Robert C. Review of Barbara N. McLennan, ed., *Crime in Urban Society.* 1189
Jados, Stanley S. Review of Juraj Andrassy, *International Law and the Resources of the Sea.* 1242
Jahnige, Thomas P. and Sheldon Goldman. *The Federal Courts as a Political System.* Reviewed by C. Herman Pritchett. 1183
Janda, Kenneth. *Data Processing: Applications to Political Research.* Reviewed by Charles Lewis Taylor. 1165
Janda, Kenneth. Review of Ralph L. Bisco, ed., *Data Bases, Computers, and the Social Sciences.* 785
Janowitz, Morris. Book Note on James A. Donovan, *Militarism, USA.* 210
The Japanese Challenge. Robert Guillain. Reviewed by Hans H. Baerwald. 1209
Japanese Society. Chie Nakane. Reviewed by Tosh Lee. 1223
Jenks, C. Wilfred. *The World Beyond the Charter in Historical Perspective: A Tentative Synthesis of Four Stages of World Organization.* Reviewed by Robert W. Gregg. 874
Jennings, M. Kent and Richard G. Niemi. *The Division of Political Labor Between Mothers and Fathers.* 69
Jensen, Lloyd. Review of *Problems of Modern Strategy.* With a Foreword by Alastair Buchan. 867
John Stuart Mill. Alan Ryan. Reviewed by John D. Lewis. 198
Johnpoll, Bernard K. *Pacifist's Progress:*

Norman Thomas and the Decline of American Socialism. Reviewed by Mulford Q. Sibley. 807
Johnson, Chalmers, ed. *Change in Communist Systems.* Reviewed by Melvin Croan. 547
Johnson, Gerald W. *Research Note on Political Correlates of Voter Participation: A Deviant Case Analysis.* 768
Johnson, Oakley C. *Robert Owen in the United States.* Reviewed by Harold V. Rhodes. 509
Johnston, Ray E. Review of Mayer N. Zald, *Organizational Change: The Political Economy of the YMCA.* 201
Johnston, Scott D. Review of Uri Avnery, *Israel Without Zionists: A Plea for Peace in the Middle East.* 866
Joint Development Group. *The Postwar Development of the Republic of Vietnam: Policies and Programs.* Reviewed by Allan E. Goodman. 1213
Jones, Charles O. Review of Abraham Holtzman, *Legislative Liaison: Executive Leadership in Congress.* 805
Judgment on Nuremberg. William J. Bosch. Reviewed by Roberta S. Sigel. 796
Judicial Biography and the Behavioral Persuasion. J. Woodford Howard, Jr. 704

Kahn, Herman. *The Emerging Japanese Superstate, Challenge and Response.* Reviewed by Hans H. Baerwald. 1209
Kalahari Village Politics: An African Democracy. Adam Kuper. Reviewed by Ronald Cohen. 1216
Kaplan, Morton. *Macropolitics.* Reviewed by William Welch. 510
Karl Radek: The Last Internationalist. Warren Lerner. Reviewed by Dan N. Jacobs. 552
Kay, David A. Review of Sydney D. Bailey, *Voting in the Security Council.* 1243
Kay, David A. *The New Nations in the United Nations. 1960–1967.* Reviewed by Edward T. Rowe. 875
Keohane, Nannerl O. Review of E. Tangye Lean, *The Napoleonists: A Study in Political Disaffection.* 1217
Kerber, Linda K. *Federalists in Dissent: Imagery and Ideology in Jeffersonian America.* Reviewed by Gordon Lloyd. . . 524
Kim, Byong Sik. *Modern Korea: The Socialist North, Revolutionary Perspectives in the South, and Unification.* Reviewed by John C. H. Oh. 1215
Kim, Chong Lim. *Socio-Economic Development and Political Democracy in Japanese Prefectures.* 184
Kirkpatrick, Evron M. *Toward a More Responsible Two-Party System: Political Science, Policy Science, or Pseudo-Science?* 965
Kitagawa, Joseph M., ed., *Understanding Modern China.* Reviewed by Thomas P. Bernstein. 549
Klein, George. Review of Paul L. Horecky, *East Central Europe: A Guide to Basic Publications.* 1211
Kochanek, Stanley A. Review of Robert W. Stern, *The Process of Opposition in India.* 1231
Kogan, Norman. Review of F. Roy Willis, *Italy Chooses Europe.* 1241
Kohlmeier, Louis M., Jr. *The Regulators: Watchdog Agencies and the Public Interest.* Reviewed by David M. Welborn. . . . 1186
Kornberg, Allan and Robert C. Frasure, *Policy Differences in British Parliamentary Parties.* 694
Kothari, Shanti and Ramashray Roy, *Relations Between Politicians and Administrators at the District Level.* Book Note by Paul Wallace. 223
Kovel, Joel. *White Racism: A Psychohistory.* Review of Donald G. Baker. 1187
Kovrig, Bennett. Review of Iván Völgyes, *The Hungarian Soviet Republic, 1919.* . . 857
Kramer, Gerald H. *Communication* on Casstevens "A Theorem about Voting" (March 1968). 188
Kramer, Gerald H. *Short-Term Fluctuations in U.S. Voting Behavior, 1896–1964.* 131
Kriegel, Annie. *Les Communistes français.* Book note by William G. Andrews. 221
Kronenberg, Philip S. Book Note on Oscar Grusky and George A. Miller, eds., *The Sociology of Organizations: Basic Studies.* 203
Kuper, Adam. *Kalahari Village Politics: An African Democracy.* Reviewed by Ronald Cohen. 1216

Ladd, Everett Carll, Jr. *American Political Parties: Social Change and Political Response.* Reviewed by William J. Crotty. . . 808
Laird, Roy D. *The Soviet Paradigm: An Experiment in Creating a Monohierarchical Society.* Reviewed by Cyril E. Black. . . . 841
Language Conflict and National Development: Group Politics and National Language Policy in India. Jyotirindra Das Gupta. Reviewed by Baldev Raj Nayar. . . 830
Lane, David. *The Roots of Russian Communism, A Social and Historical Study of Russian Social-Democracy 1898–1907.* Reviewed by Thornton Anderson. 551
Lane, David S. Review of Robert J. Osborn, *Soviet Social Policies: Welfare, Equality and Community.* 1224
La Presenza Sociale del PCI è della DC.

Luciano Brunelli et al. Reviewed by Alan J. Stern. 1197

Latey, Maurice. *Patterns of Tyranny*. Reviewed by Daniel Lerner. 842

Latin America in Transition: Problems in Training and Research. Stanley R. Ross, ed. Reviewed by John D. Martz. 851

The Latin Americans. Victor Alba. Book Note by Cole Blasier. 219

Lattimore, Owen. Review of Teresa Rakowska-Harmstone, *Russia and Nationalism in Central Asia: The Case of Tadzhikistan*. 1254

L'Avenir du Parti Communiste français. Waldeck Rochet. Book Note by William G. Andrews. 221

Leach, Richard H. Review of Daniel J. Elazar, *Cities of the Prairie. The Metropolitan Frontier and American Politics*. 521

Lean, E. Tangye. *The Napoleonists: A Study in Political Disaffection*. Reviewed by Nannerl O. Keohane. 1217

Lear, Elmer N. Review of Morroe Berger, *Islam in Egypt Today: Social and Political Aspects of Popular Religion*. 534

Le communisme en France et en Italie. Vol. I: Le communisme en France. Frédéric Bon et al. Book Note by William G. Andrews. 221

Lee, Tosh. Review of Chie Nakane, *Japanese Society*. 1223

Leemans, A. F. *Changing Patterns of Local Government*. Reviewed by Francine F. Rabinovitz. 1212

Lefebvre, Henri. *The Explosion: Marxism and the French Upheaval*. Reviewed by A. Belden Fields. 843

Legislative Liaison: Executive Leadership in Congress. Abraham Holtzman. Reviewed by Charles O. Jones. 805

Legvold, Robert. *Soviet Policy in West Africa*. Reviewed by David T. Cattell. 877

LeMelle, Tilden J. and George W. Shepherd, eds. *Race Among Nations: A Conceptual Approach*. Reviewed by Pierre L. van den Berghe. 854

Leng, Shao-chuan. Review of Harold Z. Schiffrin, *Sun Yat-sen and the Origins of the Chinese Revolution*. 853

Lerner, Daniel. Review of Maurice Latey, *Patterns of Tyranny*. 842

Lerner, Warren. *Karl Radek: The Last Internationalist*. Reviewed by Dan N. Jacobs. 552

Les Communistes français. Annie Kriegel. Book Note by William G. Andrews. 221

Lev, Daniel. Review of R. William Liddle, *Ethnicity, Party and National Integration: An Indonesian Case Study*. 555

Levi, Werner. Review of Joseph Frankel, *National Interest*. 587

Levine, Donald N. Review of Robert L. Hess, *Ethiopia; The Modernization of Autocracy*. 836

Levine, E. Lester and James W. Clarke. *Marijuana Use, Social Discontent, and Political Alienation: A Study of High School Youth*. 120

Levinson, Jerome and Juan de Onís. *The Alliance That Lost its Way: A Critical Report on the Alliance for Progress*. Reviewed by Michael G. Webster. 1250

Levitt, Morris. Review of Donald Canty, *A Single Society: Alternatives to Urban Apartheid;* and William K. Tabb, *The Political Economy of the Black Ghetto*. 1176

Levy, Frank and Edwin M. Truman, *Toward a Rational Theory of Decentralization: Another View*. 172

Lewis, John D. Review of Alan Ryan, *John Stuart Mill*. 198

Lewis, John D. Review of Dennis F. Thompson, *The Democratic Citizen: Social Science and Democratic Theory in the Twentieth Century*. 1172

Lewis, John Wilson, ed. *Party Leadership and Revolutionary Power in China*. Reviewed by Chün-tu Hsüeh. 553

Lewis, Sherman and Allan G. Austin. *Urban Government for Metropolitan Lima*. Reviewed by David Collier. 820

Liang, Hsi-huey. *The Berlin Police Force in the Weimar Republic*. Reviewed by Ronald F. Bunn. 1218

Liddle, R. William. *Ethnicity, Party and National Integration: An Indonesian Case Study*. Reviewed by Daniel Lev. 555

Liebman, Arthur. *The Politics of Puerto Rican University Students*. Reviewed by Leila A. Sussmann. 1220

Lieber, Robert J. *British Politics and European Unity: Parties, Elites and Pressure Groups*. Reviewed by D. J. Wenden. 1219

Lijphart, Arend. *Comparative Politics and the Comparative Method*. 682

The Limits of Hitler's Power. Edward N. Peterson. Reviewed by Peter W. Nixdorff. 849

Lippmann, Walter. *Early Writings* and *Men of Destiny*. Reviewed by Anwar Syed. 512

Lipset, Seymour M. and Earl Raab. *The Politics of Unreason: Right-Wing Extremism in America, 1790–1970*. Reviewed by Elizabeth W. Marvick and Dwaine Marvick. 525

Litt, Edgar. Review of Humbert S. Nelli, *The Italians in Chicago 1880–1930: A Study in Ethnic Mobility*. 1191

The Little Legislatures: Committees of Congress. George N. Goodwin, Jr. Book Note by John F. Manley. 212

Lloyd, Gordon. Review of Linda K. Kerber, *Federalists in Dissent: Imagery and Ideology in Jeffersonian America.* 524

Lloyd, Trevor. Review of David Goldsworthy. *Colonial Issues in British Politics, 1945–1961.* 1208

Loewenstein, Karl. Review of Klaus von Beyme, *Die Parlamentarischen Regierungssysteme in Europa.* 858

Lord and Peasant in Peru: A Paradigm of Political and Social Change. F. LaMond Tullis. Reviewed by Carlos A. Astiz. 855

Lorwin, Val R. Review of Byron Criddle, *Socialists and European Integration: A Study of the French Socialist Party.* 827

Los Que Mandan. José Luis de Imaz. Reviewed by Peter Ranis. 546

Loveridge, Ronald O. Review of J. Clarence Davies III, *Politics of Pollution.* 1179

Lubomudrov, Slava. Review of G. F. Hudson, *Fifty Years of Communism: Theory and Practice, 1917–1967.* 838

Ludz, Peter Christian. *Parteielite Im Wandel.* Reviewed by John M. Starrels. 555

Macdonald, H. Malcolm. Review of Peter Calvert, *Revolution.* 825

MacGaffey, Wyatt. *Custom and Government in the Lower Congo.* Reviewed by M. Crawford Young. 557

MacRae, Duncan, Jr. *Scientific Communication, Ethical Argument, and Public Policy.* 38

Macropolitics. Morton Kaplan. Reviewed by William Welch. 510

Malayan Federalism 1945–1963: A Study of Federal Problems in a Plural Society. B. Simandjuntak. Book Note by Marvin L. Rogers. 230

Managing the Modern City. James M. Banovetz, ed. Reviewed by Arthur Naftalin. 1174

Manley, John F. Book Note on George N. Goodwin, Jr., *The Little Legislatures: Committees of Congress.* 212

Man's Power: A Biased Guide to Political Thought and Action. Kalman H. Silvert. Reviewed by A. F. K. Organski. 566

Mansfield, Harvey C., Jr. *Hobbes and the Science of Indirect Government.* 97

Mao Papers: Anthology and Bibliography. Jerome Ch'en, ed. Reviewed by Richard Baum. 1200

Marijuana Use, Social Discontent, and Political Alienation: A Study of High School Youth. James W. Clarke and E. Lester Levine. 120

Marmor, Theodore R. *On Comparing Income Maintenance Alternatives.* 83

Marshall in China, John Robinson Beal. Reviewed by O. Edmund Clubb. 532

Martin, Eileen and Samuel Humes. *The Structure of Local Government.* Reviewed by Francine F. Rabinovitz. 1212

Martin, Robert E. Review of Theodore Draper, *The Rediscovery of Black Nationalism.* 1180

Martz, John D. Review of Stanley R. Ross, ed., *Latin America in Transition: Problems in Training and Research.* 851

Marvick, Elizabeth W. and Dwaine. Review of Seymour M. Lipset and Earl Raab, *The Politics of Unreason: Right-Wing Extremism in America, 1790–1970.* 525

Marx and Keynes: The Limits of the Mixed Economy. Paul Mattick. Reviewed by Martin Bronfenbrenner. 789

Marx's Economics, Origin and Development. Alexander Balinky. Reviewed by Murray Wolfson. 507

Mason, Philip, ed. *India and Ceylon: Unity and Diversity. A Symposium.* Reviewed by Richard L. Park. 845

Mason, Philip. *Race Relations.* Reviewed by John E. Crow. 845

Massialas, Byron G. Review of Joseph Fischer, ed. *The Social Sciences and the Comparative Study of Educational Systems.* 1206

Matley, Ian M. *Romania; A Profile.* Review by A. G. Caranfil. 558

Mattick, Paul. *Marx & Keynes: The Limits of the Mixed Economy.* Reviewed by Martin Bronfenbrenner. 789

Mayer, Lawrence S., *Communication* in reply to communications by Kramer and Casstevens (March 1971). 779

Mayer, Lawrence S. *A Note on "An Axiomatic Model of Voting Bodies."* 764

Mazrui, Ali A. and Robert I. Rotberg, eds. *Protest and Power in Black Africa.* Book Note by Christian P. Potholm. 229

McCall, Charles. Review of David Adamany, *Financing Politics: Recent Wisconsin Elections.* 516

McClellan, Robert. *The Heathen Chinee, A Study of American Attitudes Toward China, 1890–1905.* Reviewed by Harold R. Isaacs. 1189

McDaniel, Gerald R. Review of Stanley V. Anderson, ed., *Ombudsmen for American Government?* 517

McHale, Vincent E. Review of Harvey G. Simmons, *French Socialists in Search of a Role, 1956–1967.* 199

McHale, Vincent E. Review of Philip M. Williams, *French Politicians and Elections, 1951–1969.* 859

McLaughlin, Charles H. Review of Clive Parry, LL.D., ed., *The Consolidated*

Treaty Series, 1648–1918. 588
McLennan, Barbara N., ed. *Crime in Urban Society*. Reviewed by Robert C. Jacobs. 1189
Meltsner, Arnold J. Review of L. L. Ecker-Racz, *The Politics and Economics of State-Local Finance.* 801
Melson, Robert. *Ideology and Inconsistency: The Cross-Pressured Nigerian Worker.* 161
Mendl, Wolf. *Deterrence and Persuasion.* Reviewed by Stanley Hoffmann. 878
Men of Destiny. Walter Lippmann. Reviewed by Anwar Syed. 512
Merelman, Richard M. *The Development of Policy Thinking in Adolescence.* 1033
The Methodology of Comparative Research. Robert T. Holt and John E. Turner, eds. Reviewed by G. Bingham Powell, Jr. 545
The Mexican Revolution: Federal Expenditure and Social Change Since 1910. James W. Wilkie. Reviewed by Charles W. Anderson. 572
Micro-Macro Political Analysis. Heinz Eulau. Reviewed by Michael J. Shapiro. 508
Middle East Oil: A Study in Political and Economic Controversy. George W. Stocking. Reviewed by L. Hirszowicz. 881
Mihaly, Eugene B. Review of Edward L. Bernays and Burnet Hershey, eds., *The Case for Reappraisal of U.S. Overseas Information Policies and Programs.* 577
Milenkovitch, Michael M. Review of R. Barry Farrell, ed., *Political Leadership in Eastern Europe and the Soviet Union.* 833
Militarism, USA. James A. Donovan. Book Note by Morris Janowitz. 210
Miller, Charles A. *The Supreme Court and the Uses of History*. Reviewed by David Farrelly. 1190
Miller, Delbert C. *International Community Power Structures: Comparative Studies of Four World Cities.* Reviewed by Robert T. Daland. 1251
Miller, Edward and E. B. Fryde, eds. *Historical Studies of the English Parliament.* Reviewed by John Clarke Adams. 1208
Miller, George A. and Oscar Grusky, eds. *The Sociology of Organizations: Basic Studies.* Book Note by Philip Kronenberg. 203
Miller, Robert F. *One Hundred Thousand Tractors: The MTS and the Development of Controls in Soviet Agriculture.* Reviewed by Anna Elizabeth Powell. 196
Mills, Richard M. Review of Galina G. Boichenko, *The Political Organization of the USA: Social Institutions and their Interaction With the State.* 795
Mink, Louis O. Review of Haskell Fain, *Between Philosophy and History: The Resurrection of Speculative Philosophy of History Within the Analytic Tradition.* 1164
Misra, K. P. and S. P. Varma, eds. *Foreign Policies in South Asia.* Reviewed by Arthur Stein. 882
Modern China's Search for a Political Form. Jack Gray, ed. Book Note by Derek J. Waller. 226
Modern Korea: The Socialist North, Revolutionary Perspectives in the South, and Unification. Byong Sik Kim. Reviewed by John C. H. Oh. 1215
Moore, Clement Henry Review of John Waterbury, *The Commander of the Faithful: The Moroccan Political Elite—A Study of Segmented Politics.* 570
Moral Principles in Political Philosophy. Felix E. Oppenheim. Book Note by Alan Wertheimer. 204
Moreland, Lois. Book Note on Adam Carlyle Breckenridge, *The Right to Privacy.* 207
Moreno, Francisco José. Review of Ben G. Burnett, *Political Groups in Chile: The Dialogue Between Order and Change.* 1198
Morgenthau, Hans J. *Truth and Power: Essays of a Decade, 1960–70.* Reviewed by Wolfram F. Hanreider. 879
Mörner, Magnus, ed. *Race and Class in Latin America.* Reviewed by Kalman H. Silvert. 1221
Moskos, Charles C., Jr. *The American Enlisted Man.* Reviewed by M. D. Feld. 803
The Mourides of Senegal; The Political and Economic Organization of an Islamic Brotherhood. Donal Cruise O'Brien. Reviewed by Lucy C. Behrman. 1202
Moynihan, Daniel P., ed. *Toward a National Urban Policy.* Reviewed by Eric C. Freund. 810
Mueller, John E. *Trends in Popular Support for the Wars in Korea and Vietnam.* 358
Munger, Frank J. Review of Basil Chubb, *The Government and Politics of Ireland.* 538
The Municipal Voter: Voting and Nonvoting in City Elections. Howard D. Hamilton 1135
Murphy, James T. Book Note on Bernard C. Hennessey, *Political Internships: Theory, Practice, Evaluation.* 215
Murphy, Thomas P. and F. Gerald Brown, eds. *Emerging Patterns in Urban Administration.* Reviewed by Joseph M. Heikoff. 797
Murray, D. J. *The Governmental System in Southern Rhodesia.* Reviewed by James R. Scarritt. 559
Muslim Brotherhoods and Politics in Senegal. Lucy C. Behrman. Reviewed by Donal Cruise O'Brien. 533
Mussolini's Early Diplomacy. Alan Cassels. Reviewed by Alberto Aquarone. 869

Myth and Reality in Late Eighteenth Century British Politics. Ian R. Christie. Reviewed by J. G. A. Pocock. 826

Naftalin, Arthur. Review of James M. Banovetz, ed. *Managing the Modern City.* 1174
Nagle, John David. *The National Democratic Party: Right Radicalism in the Federal Republic of Germany.* Reviewed by Kurt P. Tauber. 247
Nakane, Chie. *Japanese Society.* Reviewed by Tosh Lee. 1223
The Napoleonists: A Study in Political Disaffection. E. Tangye Lean. Reviewed by Nannerl O. Keohane. 1217
National Communication and Language Policy in India. Baldev Raj Nayar. Reviewed by Robert W. Stern. 560
The National Democratic Party: Right Radicalism in the Federal Republic of Germany. John David Nagle. Reviewed by Kurt P. Tauber. 847
The National Executive Branch: An Introduction. James W. Davis, Jr. Reviewed by Thomas E. Cronin. 799
National Interest. Joseph Frankel. Reviewed by Werner Levi. 587
Nationalism and Economic Development in Ghana. Roger Genoud. Book Note by David J. Finlay. 225
Nayar, Baldev Raj. *National Communication and Language Policy in India.* Reviewed by Robert W. Stern. 560
Nayar, Baldev Raj. Review of Jyotirindra Das Gupta, *Language Conflict and National Development: Group Politics and National Language Policy in India.* 830
Negroes and the Great Depression: The Problem of Economic Recovery. Raymond Wolters. Reviewed by John E. Crow. 528
Nelli, Humbert S. *The Italians in Chicago 1880–1930: A Study in Ethnic Mobility.* Reviewed by Edgar Litt. 1191
Nettler, Gwynn. *Explanations.* Reviewed by Robert Brown. 1167
Neustadt, Richard E. *Alliance Politics.* Reviewed by Francis E. Rourke. 880
The New Age of American Foreign Policy. Frederick H. Hartmann. Reviewed by David V. Edwards. 1249
The New Nations in the United Nations, 1960–1967. David A. Kay. Reviewed by Edward T. Rowe. 875
Nexon, David. *Asymmetry in the Political System: Occasional Activists in the Republican and Democratic Parties, 1956–1964.* 716
Niemi, Richard G. and M. Kent Jennings, *The Division of Political Labor Between Mothers and Fathers.* 69
Nixdorff, Peter W. Reviews of Dietrich Orlow, *The History of the Nazi Party: 1919–1933;* and Edward N. Peterson, *The Limits of Hitler's Power.* 849
Nixon Agonistes: The Crisis of the Self-Made Man. Garry Wills. Reviewed by James MacGregor Burns. 818
No Exit From Vietnam. Robert Thomson. Reviewed by Edward Reynolds Wright. . . 1232
Nonaligned Black Africa: An International Subsystem. George W. Shepherd, Jr. Reviewed by Amry Vandenbosch. 1255
Nondecisions and the Study of Local Politics. Raymond E. Wolfinger. 1063
Noonan, Lowell T. *France: The Politics of Continuity in Change.* Reviewed by Suzanne Berger. 848
A Note on "An Axiomatic Model of Voting Bodies." Lawrence S. Mayer. 764
Nuclear Diplomacy, The First Twenty-Five Years. George H. Quester. Reviewed by Simon Serfaty. 1253
Nuremberg and Vietnam: an American Tragedy. Telford Taylor. Reviewed by John H. E. Fried. 1257

O'Brien, Donal Cruise. *The Mourides of Senegal; The Political and Economic Organization of an Islamic Brotherhood.* Reviewed by Lucy C. Behrman. 1202
O'Brien, Donal Cruise. Review of Lucy C. Behrman, *Muslim Brotherhoods and Politics in Senegal.* 533
Of Laws in General. Jeremy Bentham. Book Note by Michael Barkun. 202
Oh, John C. H. Review of Byong Sik Kim, *Modern Korea: The Socialist North, Revolutionary Perspectives in the South, and Unification.* 1215
Ojukwu, Odumegwu. *Biafra: Selected Speeches and Random Thoughts of C. Ojukwu, with Diaries of Events.* Book Note by William John Hanna. 229
O'Leary, Michael K. and Steven J. Brams. *Comment on Mayer's "A Note on 'An Axiomatic Model of Voting Bodies'"* 766
Ombudsmen for American Government? Stanley V. Anderson, ed. Reviewed by Gerald R. McDaniel. 517
On Comparing Income Maintenance Alternatives. Theodore R. Marmor. 83
On Political Theory and Political Action. Karl W. Deutsch. 11
On the "Equilibrium" Division of the Vote. Duff Spafford. 180
Opinion Leaders and the Mass Media in Rural Egypt: A Reconsideration of the

Two-Step Flow of Communications Hypothesis. Iliya F. Harik. 731
On the Medieval Origins of the Modern State. Joseph R. Strayer. Reviewed by Antony J. Black. 1171
O'Neil, Robert M. *The Price of Dependency: Civil Liberties in the Welfare State.* Reviewed by Paul F. deLespinasse. 812
One Hundred Thousand Tractors: The MTS and the Development of Controls in Soviet Agriculture. Robert F. Miller. Reviewed by Anna Elizabeth Powell. 196
Oppenheim, Felix E. *Comment: Defense of Noncognitivism Defended.* 1115
Oppenheim's Defense of Noncognitivism. Donald VanDeVeer. 1105
Oppenheim, Felix E. *Moral Principles in Political Philosophy.* Book Note by Alan Wertheimer. 204
Opposition in the U.S.S.R., 1917–1967. Roland Gaucher. Reviewed by George Fischer. 543
Ordeshook, Peter C. *Rejoinder to Shubik's "Comment."* 1146
Ordeshook, Peter C. *Pareto Optimality in Electoral Competition.* 1141
Organizational Change: The Political Economy of the YMCA. Mayer N. Zald. Reviewed by Ray E. Johnston. 201
Organski, A. F. K. Review of Kalman H. Silvert, *Man's Power: A Biased Guide to Political Thought and Action.* 566
Orlow, Dietrich. *The History of the Nazi Party: 1919–1933.* Reviewed by Peter W. Nixdorff. 849
Osborn, Robert J. *Soviet Social Policies: Welfare, Equality and Community.* Reviewed by David S. Lane. 1224
Oswald, J. Gregory and Anthony J. Strover, eds. *The Soviet Union and Latin America.* Reviewed by George I. Blanksten. 837
Ottaway, David and Marina. *Algeria: The Politics of a Socialist Revolution.* Book Note by Brian Weinstein. 228
Outland, John W. Review of William O. Chittick, *State Department, Press, and Pressure Groups: A Role Analysis.* 1178

Pacifist's Progress: Norman Thomas and the Decline of American Socialism. Bernard K. Johnpoll. Reviewed by Mulford Q. Sibley. 807
Padover, Saul K. Review of Robert E. Brown, *Carl Becker on History and the American Revolution.* 518
Parenti, Michael, *Communication* on review of his book "The Anti-Communist Impulse." 780
Pareto Optimality in Electoral Competition. Peter C. Ordeshook. 1141
Park, Richard L. Review of Philip Mason, ed., *India and Ceylon: Unity and Diversity. A Symposium.* 845
Parry, Clive, LL.D., ed. *The Consolidated Treaty Series, 1648–1918.* Reviewed by Charles H. McLaughlin. 588
Parry, Geraint. *Political Elites.* Reviewed by Robert Presthus. 1225
Parteielite Im Wandel. Peter Christian Ludz. Reviewed by John M. Starrels. 555
Party and Constituency: Pressures on Congress. Julius Turner. (Revised Edition by Edward V. Schneier, Jr.) Reviewed by Frank J. Sorauf. 817
Party Effort and Its Impact on the Vote. William J. Crotty. 439
Party Systems and Government Stability. Michael Taylor and V. M. Herman. 28
Party Leadership and Revolutionary Power in China. John Wilson Lewis, ed. Reviewed by Chün-tu Hsüeh. 553
Patterns of Political Participation in Italy. Giorgio Galli and Alfonso Prandi. Reviewed by Samuel H. Barnes. 835
Patterns of Tyranny. Maurice Latey. Reviewed by Daniel Lerner. 842
Patterson, Samuel C. Book Note on Leonard Ruchelman, *Political Careers: Recruitment Through the Legislature.* 217
Peabody, Robert L. Review of Lewis Anthony Dexter, *The Sociology and Politics of Congress.* 1182
Pelowski, Alan L. and James A. Caporaso, *Economic and Political Integration in Europe: A Quasi-Experimental Analysis.* ... 418
Peterson, Edward N. *The Limits of Hitler's Power.* Reviewed by Peter W. Nixdorff. .. 849
Platt, George M. Review of Richard S. Wheeler, *The Politics of Pakistan: A Constitutional Quest.* 1238
The Play Theory of Mass Communication. William Stephenson. Reviewed by Timothy Alden Williams. 514
Ploss, Sidney I. Review of Lazar Volin, *A Century of Russian Agriculture: From Alexander II to Khrushchev.* 1235
Pocock, J. G. A. Review of Ian R. Christie, *Myth and Reality in Late Eighteenth Century British Politics.* 826
Policy Differences in British Parliamentary Parties. Allan Kornberg and Robert C. Frasure. 694
Political Careers: Recruitment Through the Legislature. Leonard Ruchelman. Book Note by Samuel C. Patterson. 217
Political Change in California: Critical Elec-

tions and Social Movements, 1890–1966. Michael Paul Rogin and John L. Shover. Reviewed by John A. Vieg. 1192
Political Democracy as a Property of Political Institutions. Douglas W. Rae. 111
Political Development. Gabriel A. Almond. Reviewed by Robert E. Dowse. 529
The Political Economy of the Black Ghetto. William K. Tabb. Reviewed by Morris Levitt. 1176
The Political Economy of Communist China. Jan S. Prybyla. Reviewed by Yuan-li Wu. 1226
The Political Economy of Modern Spain: Policy-Making in an Authoritarian System. Charles W. Anderson. Reviewed by Joan C. Ullman. 1196
Political Elites. Geraint Parry. Reviewed by Robert Presthus. 1225
Political Ethos Revisited. James Q. Wilson and Edward C. Banfield. 1048
Political Groups in Chile: The Dialogue Between Order and Change. Ben G. Burnett. Reviewed by Francisco José Moreno. 1198
Political Integration by Jurisprudence: The Work of the Court of Justice of the European Communities in European Political Integration. Andrew Wilson Green. Reviewed by Salo Engel. 873
Political Internships: Theory, Practice, Evaluation. Bernard C. Hennessey. Book note by James T. Murphy. 215
Political Leadership in Eastern Europe and the Soviet Union. R. Barry Farrell, ed. Reviewed by Michael M. Milenkovitch. 833
The Political Organization of the USA: Social Institutions and their Interaction With the State. Galina G. Boichenko. Reviewed by Richard M. Mills. 795
The Politics and Economics of State-Local Finance. L. L. Ecker-Racz. Reviewed by Arnold J. Meltsner. 801
Politics and Foreign Policy in Australia. Henry S. Albinski. Book Note by Russell H. Barrett. 219
Politics as the Master Science: From Plato to Mao. Herbert J. Spiro. Reviewed by Christian Bay. 792
Politics in Sierra Leone 1947–1967. John R. Cartwright. Reviewed by Clement Cottingham. 1199
The Politics of Compromise: State and Religion in Israel. Ervin Birnbaum. Reviewed by Norman L. Zucker. 821
The Politics of Federal Courts: Lower Courts in the United States. Richard J. Richardson and Kenneth N. Vines. Reviewed by George F. Cole. 814
The Politics of Nepal: Persistence and Change in an Asian Monarchy. Leo E. Rose and Margaret W. Fisher. Reviewed by Merrill R. Goodall. 851
The Politics of Pakistan: A Constitutional Quest. Richard S. Wheeler. Reviewed by George M. Platt. 1238
Politics of Pollution. J. Clarence Davies III. Reviewed by Ronald O. Loveridge. 1179
The Politics of Puerto Rican University Students. Arthur Liebman. Reviewed by Leila A. Sussmann. 1220
Politics of Southern Equality: Law and Social Change in a Mississippi County. Frederick M. Wirt. Reviewed by Robert J. Steamer. 1194
The Politics of Struggle: The Communist Front and Political Warfare. James D. Atkinson. Reviewed by Pi-chao Chen. 576
The Politics of Unreason: Right-Wing Extremism in America, 1790–1970. Seymour M. Lipset and Earl Raab. Reviewed by Elizabeth W. Marvick and Dwaine Marvick. 525
Politics, Personality and Social Science in the Twentieth Century: Essays In Honor of Harold D. Lasswell. Arnold A. Rogow, ed. Reviewed by Robert H. Horwitz. 1169
Politics, Position, and Power. Harold Seidman. Reviewed by Peter Woll. 815
The Postwar Development of the Republic of Vietnam; Policies and Programs. Joint Development Group. Reviewed by Allen E. Goodman. 1213
Potholm, Christian P. Book Note on Robert I. Rotberg and Ali A. Mazrui, eds., *Protest and Power in Black Africa.* 229
Pounds, Norman J. G. *Eastern Europe.* Reviewed by Peter A. Toma. 562
Powell, Anna Elizabeth. Review of Robert F. Miller, *One Hundred Thousand Tractors: The MTS and the Development of Controls in Soviet Agriculture.* 196
Powell, G. Bingham, Jr. Review of Robert T. Holt and John E. Turner, eds., *The Methodology of Comparative Research.* 545
Power and Intention. D. M. White. 749
Power and Society in Contemporary Peru. Francois Bourricaud. Reviewed by Robert E. Scott. 536
Prandi, Alfonso and Giorgio Galli. *Patterns of Political Participation in Italy.* Reviewed by Samuel H. Barnes. 835
Predicting Voting Patterns in the General Assembly. Jack E. Vincent. 471
Prelude to Revolution: France in May 1968. Daniel Singer. Reviewed by A. Belden Fields. 843
Prescription and Description in Political Thought: The Case for Hobbes. Blair Campbell. 376

Presthus, Robert. Review of Geraint Parry, *Political Elites.* 1225
Preventing the Spread of Nuclear Weapons. C. F. Barnaby. Reviewed by Ciro E. Zoppo. 1243
The Price of Dependency: Civil Liberties in the Welfare State. Robert M. O'Neil. Reviewed by Paul F. deLespinasse. 812
A Primer on Simulation and Gaming. Richard F. Barton. Reviewed by Charles D. Elder. 1158
Pritchett, C. Herman. Review of Sheldon Goldman and Thomas P. Jahnige. *The Federal Courts as a Political System.* ... 1183
Private Wants, Public Means: An Economic Analysis of the Desirable Scope of Government. Gordon Tullock. Reviewed by Mark Sproule-Jones. 515
Private Wealth and Public Education. John E. Coons, William H. Clune and Stephen D. Sugarman. Reviewed by Earl F. Cheit. 798
Problems of Modern Strategy. With a Foreward by Alastair Buchan. Reviewed by Lloyd Jensen. 867
The Process of Opposition in India. Robert W. Stern. Reviewed by Stanley A. Kochanek. 1231
Protest and Power in Black Africa. Robert I. Rotberg and Ali A. Mazrui, eds. Book Note by Christian P. Potholm. 229
Prybyla, Jan S. *The Political Economy of Communist China.* Reviewed by Yuan-li Wu. 1226
Przeworski, Adam and Glaucio A. D. Soares. *Theories in Search of a Curve: A Contextual Interpretation of Left Vote.* . 51
Psychological Sources of Political Belief: Self-Esteem and Isolationist Attitudes. Paul M. Sniderman and Jack Citrin. 401
Puchala, Donald J. Review of Louis Armand and Michel Drancourt, *The European Challenge. A Detailed Blueprint on How Europe can Use the Tools of the New Technology to Keep Pace With The 1970s.* 575
Putnam, Robert D. *Studying Elite Political Culture: The Case of 'Ideology.'* 651

Quester, George H. *Nuclear Diplomacy, The First Twenty-Five Years.* Reviewed by Simon Serfaty. 1253

Raab, Earl and Seymour M. Lipset. *The Politics of Unreason: Right-Wing Extremism in America, 1790–1970.* Reviewed by Elizabeth W. Marvick and Dwaine Marvick. 525
Ra'anan, Uri. *The USSR Arms the Third World: Case Studies in Soviet Foreign Policy.* Reviewed by Robert G. Wesson. 562
Rabinowitch, Eugene, Review of George Fischer, ed., *Science and Ideology in the Soviet Union.* 1204
Rabinovitz, Francine F. Review of Samuel Humes and Eileen Martin, *The Structure of Local Government;* and A. F. Leemans, *Changing Patterns of Local Government.* 1212
Race Among Nations: A Conceptual Approach. George W. Shepherd and Tilden J. LeMelle, eds. Reviewed by Pierre L. van den Berghe. 854
Race and Class in Latin America. Magnus Mörner, ed. Reviewed by Kalman H. Silvert. 1221
Race Relations in Sociological Theory. John Rex. Reviewed by John E. Crow. 845
Race Relations. Philip Mason. Reviewed by John E. Crow. 845
Radosh, Ronald. *American Labor and United States Foreign Policy.* Reviewed by Robert W. Hattery. 813
Rae, Douglas W. and Michael Taylor. *The Analysis of Political Cleavages.* Reviewed by Kenneth A. Shepsle. 790
Rae, Douglas W. *Political Democracy as a Property of Political Institutions.* 111
Rahul, Ram. *The Himalaya Borderland.* Reviewed by Kunjar M. Sharma. 1228
Rakowska-Harmstone, Teresa. *Russia and Nationalism in Central Asia: the Case of Tadzhikistan.* Reviewed by Owen Lattimore. 1254
Ranis, Peter. Review of José Luis de Imaz, *Los Que Mandan.* 546
Ransom, Harry Howe. Review of Michael Elliott-Bateman, ed., *The Fourth Dimension of Warfare.* 1248
Rebellion, Racism, and Representation: The Adam Clayton Powell Case and Its Antecedents. P. Alan Dionisopoulos. Reviewed by Robert S. Getz. 800
The Rediscovery of Black Nationalism. Theodore Draper. Reviewed by Robert E. Martin. 1180
Refugees South of the Sahara: An African Dilemma. Hugh C. Brooks and Yassin El-Ayouty, eds. Reviewed by Channing B. Richardson. 537
Régis Debray and the Latin-American Revolution. Leo Huberman and Paul M. Sweezy, eds. Reviewed by George I. Blanksten. 837
The Regulators: Watchdog Agencies and the Public Interest. Louis M. Kohlmeier, Jr. Reviewed by David M. Welborn. 1186
Reisman, W. M. Review of William I. Shore, *Fact-finding in the Maintenance of International Peace.* 1256

Rejoinder to Frey's "Comment." Raymond E. Wolfinger. 1102

Rejoinder to Oppenheim's "Comment." Donald VanDeVeer. 1117

Rejoinder to Riker's "Comment." Robert Lyle Butterworth. 747

Rejoinder to Shubik's "Comment." Peter C. Ordeshook. 1146

Relations Between Politicians and Administrators at the District Level. Shanti Kothari and Ramashray Roy. Book Note by Paul Wallace. 223

RePass, David E. *Issue Salience and Party Choice.* 389

Research Note on Political Correlates of Voter Participation: A Deviant Case Analysis. Gerald W. Johnson. 768

A Research Note on the Size of Winning Coalitions. Robert Lyle Butterworth. 741

The Resurrection of Richard Nixon. Jules Witcover. Reviewed by James MacGregor Burns. 818

Revolution. Peter Calvert. Reviewed by H. Malcolm Macdonald. 825

Rex, John. *Race Relations in Sociological Theory.* Reviewed by John E. Crow. 845

Rhodes, Harold V. Review of Oakley C. Johnson, *Robert Owen in the United States* 509

Richardson, Channing B. Review of Hugh C. Brooks and Yassin El-Ayouty, *Refugees South of the Sahara: An African Dilemma.* 537

Richardson, Richard J. and Kenneth N. Vines. *The Politics of Federal Courts: Lower Courts in the United States.* Reviewed by George F. Cole. 814

Richter, Melvin. *Essays in Theory and History: An Approach to the Social Sciences.* Reviewed by Peter Bachrach. 1168

The Right to Privacy. Adam Carlyle Breckenridge. Book Note by Lois Moreland. 207

Riker, William H. *Comment on Butterworth, "A Research Note on the Size of Winning Coalitions."* 745

Rintala, Marvin. *Four Finns: Political Profiles.* Reviewed by John H. Hodgson. 1228

Robert Owen in the United States. Oakley C. Johnson. Reviewed by Harold V. Rhodes. 509

Rochet, Waldeck. *L'Avenir du Parti Communiste français.* Book Note by William G. Andrews. 221

Rogers, Marvin L. Book Note on B. Simandjuntak, *Malayan Federalism 1945–1963: A Study of Federal Problems in a Plural Society.* 230

Rogin, Michael Paul and John L. Shover, *Political Change in California: Critical Elections and Social Movements 1890–1966.* Reviewed by John A. Vieg. 1192

Rogow, Arnold A. *Politics, Personality and Social Science in the Twentieth Century: Essays in Honor of Harold D. Lasswell.* Reviewed by Robert H. Horwitz. 1169

Romania; A Profile. Ian M. Matley. Reviewed by A. G. Caranfil. 558

Roosevelt: The Soldier of Freedom. James MacGregor Burns. Reviewed by Harold F. Gosnell. 192

The Roots of Russian Communism, A Social and Historical Study of Russian Social-Democracy 1898–1907. David Lane. Reviewed by Thornton Anderson. 551

Rose, Leo E. and Margaret W. Fisher. *The Politics of Nepal: Persistence and Change in an Asian Monarchy.* Reviewed by Merrill R. Goodall. 851

Ross, Ralph. Review of Charles Fried, *An Anatomy of Values.* 193

Ross, Stanley R., ed. *Latin America in Transition: Problems in Training and Research.* Reviewed by John D. Martz. 851

Rotberg, Robert I. and Ali A. Mazrui, eds. *Protest and Power in Black Africa.* Book Note by Christian P. Potholm. 229

Rourke, Francis E. Review of Richard E. Neustadt, *Alliance Politics.* 880

Rowe, Edward T. Review of David A. Kay, *The New Nations in the United Nations, 1960–1967.* 875

Roy, Ramashray and Shanti Kothari. *Relations Between Politicians and Administrators at the District Level.* Book Note by Paul Wallace. 223

Rozental, Alek A. Review of Jordan C. Schreiber, *U.S. Corporate Investments in Taiwan.* 1229

Rubinstein. Alvin Z. Book Note on Sir William Hayter, *Russia and the World: A Study of Soviet Foreign Policy.* 227

Ruchelman, Leonard. *Political Careers: Recruitment Through the Legislature.* Book note by Samuel C. Patterson. 217

Rumble, Wilfrid E. Review of Anthony de Crespigny and Alan Wertheimer, eds., *Contemporary Political Theory.* 1162

Rush, Myron. Review of Brian Crozier, *Since Stalin: An Assessment of Communist Power.* 828

Rusk, Jerrold, *Communication* on criticism of his "The Effect of the Australian Ballot Reform on Split Ticket Voting: 1876–1908." 1152

Russia and the Austrian State Treaty: A Case Study of Soviet Policy in Europe. Sven Allard. Reviewed by William Welch. 863

Russia, China and the West. Isaac Deutscher.

Reviewed by Donald S. Zagoria. 541
Russia and Nationalism in Central Asia: the Case of Tadzhikistan. Teresa Rakowska-Harmstone. Reviewed by Owen Lattimore. 1254
Russia and the World: A Study of Soviet Foreign Policy. Sir William Hayter. Book Note by Alvin Z. Rubinstein. 227
Ryan, Alan. *John Stuart Mill.* Reviewed by John D. Lewis. 198

Sarbah, John Mensah. *Fanti National Constitution, 2nd ed.* Reviewed by L. Gray Cowan. 852
Sartori, Giovanni, ed. *Antologia di Scienza Politica.* Reviewed by Giuseppe Di Palma. 564
Saunders, Charles B., Jr. *Upgrading the American Police.* Reviewed by Samuel G. Chapman. 527
Scarritt, James R. Review of D. J. Murray, *The Governmental System in Southern Rhodesia.* 559
Schiffrin, Harold Z. *Sun Yat-sen and the Origins of the Chinese Revolution.* Reviewed by Shao-chuan Leng. 853
Schmidt, Helmut. *Strategie des Gleichgewichts: Deutsche Friedenspolitik und die Weltmächte.* Reviewed by Joachim Hütter. 1245
Schneider, Ronald M. Review of Octavio Ianni, *Crisis in Brazil.* 839
Schneier, Edward V., Jr. Revised Edition of Julius Turner's *Party and Constituency: Pressures on Congress.* Reviewed by Frank J. Sorauf. 817
Schreiber, Jordan C. *U.S. Corporate Investments in Taiwan.* Reviewed by Alek A. Rozental. 1229
Science and Ideology in the Soviet Union. George Fischer, ed. Reviewed by Eugene Rabinowitch. 1204
Scientific Communication, Ethical Argument, and Public Policy. Duncan MacRae, Jr. . . 38
Scott, Robert E. Book Note on Pablo González Casanova, *Democracy in Mexico.* . . 222
Scott, Robert E. Review of Francois Bourricaud, *Power and Society in Contemporary Peru.* 536
Seabury, Paul. Review of Herbert Feis, *From Trust to Terror: The Onset of the Cold War, 1945–1950.* 1249
Seidman, David, *Communication* on Hahn's "Correlates of Public Sentiment about War: Local Referenda on the Vietnam Issue." (December 1970). 1147
Seidman, Harold. *Politics, Position and Power.* Reviewed by Peter Woll. 815
Serfaty, Simon. Review of George H. Quester, *Nuclear Diplomacy, The First Twenty-Five Years.* 1253
Serious Games. Clark C. Abt. Reviewed by Charles D. Elder. 1158
Shapiro, Michael J. Review of Heinz Eulau, *Micro-Macro Political Analysis.* 508
Sharma, Kunjar M. Review of Ram Rahul, *The Himalaya Borderland.* 1228
Shepherd, George W. Jr. *Nonaligned Black Africa: An International Subsystem.* Reviewed by Amry Vandenbosch. 1255
Shepherd, George W. and Tilden J. LeMelle, eds. *Race Among Nations: A Conceptual Approach.* Reviewed by Pierre L. van den Berghe. 845
Shepsle, Kenneth A. Review of Douglas W. Rae and Michael Taylor, *The Analysis of Political Cleavages.* 790
Sherwood, John M. *Georges Mandel and the Third Republic.* Reviewed by Harvey G. Simmons. 565
Shields, Currin V. Book note on Harvey Wheeler, *Democracy in a Revolutionary Era: The Political Order Today.* 206
Shore, William I. *Fact-finding in the Maintenance of International Peace.* Reviewed by W. M. Reisman. 1256
Short-Term Fluctuations in U.S. Voting Behavior, 1896–1964. Gerald H. Kramer. . . 131
Shover, John L. and Michael Paul Rogin. *Political Change in California: Critical Elections and Social Movements 1890–1966.* Reviewed by John A. Vieg. 1192
Shubik, Martin. *Comment on Ordeshook, "Pareto Optimality in Electoral Competition."* 1146
Sibley, Mulford Q. Review of Bernard K. Johnpoll, *Pacifist's Progress: Norman Thomas and the Decline of American Socialism.* 807
Sigel, Roberta S. Review of William J. Bosch, *Judgment on Nuremberg.* 796
The Silent Revolution in Europe: Intergenerational Change in Post-Industrial Societies. Ronald Inglehart. 991
Silverstein, Josef. Review of F. S. V. Donnison, *Burma.* 542
Silvert, Kalman H. *Man's Power: A Biased Guide to Political Thought and Action.* Reviewed by A. F. K. Organski. 566
Silvert, Kalman H. Reviews of Magnus Mörner, ed., *Race and Class in Latin America;* and Ivan Vallier, *Catholicism, Social Control, and Modernization in Latin America.* 1221
Simandjuntak, B. *Malayan Federalism 1945–1963: A Study of Federal Problems in a Plural Society.* Book note by Marvin L. Rogers 230
Simmons, Harvey G. *French Socialists in Search of a Role, 1956–1967.* Reviewed by Vincent E. McHale. 199

Simmons, Harvey G. Review of John M. Sherwood, *Georges Mandel and the Third Republic*. 565
Since Stalin: An Assessment of Communist Power. Brian Crozier. Reviewed by Myron Rush. 828
Singer, Daniel. *Prelude to Revolution: France in May 1968*. Reviewed by A. Belden Fields. 843
A Single Society: Alternatives to Urban Apartheid. Donald Canty. Reviewed by Morris Levitt. 1176
Smith, Bradley F. *Heinrich Himmler—A Nazi in the Making, 1900–1926*. Reviewed by Raul Hilberg. 1230
Sniderman, Paul M. and Jack Citrin, *Psychological Sources of Political Belief: Self-Esteem and Isolationist Attitudes* 401
Soares, Glaucio A. D. and Adam Przeworski, *Theories in Search of a Curve: A Contextual Interpretation of Left Vote*. 51
The Social Sciences and the Comparative Study of Educational Systems. Joseph Fischer, ed. Reviewed by Byron G. Massialas. 1206
Socialists and European Integration: A Study of the French Socialist Party. Byron Criddle. Reviewed by Val R. Lorwin. 827
Socio-Economic Development and Political Democracy in Japanese Prefectures. Chong Lim Kim. 184
The Sociology of Organizations: Basic Studies. Oscar Grusky and George A. Miller, eds. Book Note by Philip S. Kronenberg. 203
The Sociology and Politics of Congress. Lewis Anthony Dexter. Reviewed by Robert L. Peabody. 1182
The Sōkagakkai and Mass Society. James W. White. Reviewed by James Allen Dator. 1240
Sorauf, Frank J. Review of Julius Turner, *Party and Constituency: Pressures on Congress* (Revised Edition by Edward V. Schneier, Jr.). 817
The Soviet Paradigm: An Experiment in Creating a Monohierarchical Society. Roy D. Laird. Reviewed by Cyril E. Black. 841
Soviet Policy in West Africa. Robert Legvold. Reviewed by David T. Cattell. 877
Soviet Power and Europe, 1945–1970. Thomas W. Wolfe. Reviewed by Lawrence T. Caldwell. 884
Soviet Relations With Latin America 1918–1968: A Documentary Survey. Stephen Clissold, ed. Reviewed by Herbert S. Dinerstein. 581
Soviet Social Policies: Welfare, Equality and Community. Robert J. Osborn. Reviewed by David S. Lane. 1224
The Soviet Union and Latin America. J. Gregory Oswald and Anthony J. Strover. Reviewed by George I. Blanksten. 837
Spafford, Duff. *On the "Equilibrium" Division of the Vote*. 180
Speer, Albert. *Inside the Third Reich*. Reviewed by John H. E. Fried. 568
Spiro, Herbert J. *Politics as the Master Science: From Plato to Mao*. Reviewed by Christian Bay. 792
Sproule-Jones, Mark. Review of Gordon Tullock, *Private Wants, Public Means: An Economic Analysis of the Desirable Scope of Government*. 515
Stankiewicz, W. J. ed. *In Defense of Sovereignty*. Reviewed by Robert K. Woetzel. 590
Starrels, John M. Review of Peter Christian Ludz, *Parteielite Im Wandel*. 555
State Department, Press, and Pressure Groups: A Role Analysis. William O. Chittick. Reviewed by John W. Outland. 1178
Statistical Models of Senate Roll Call Voting. John E. Jackson. 451
Steamer, Robert J. Review of Frederick M. Wirt, *Politics of Southern Equality: Law and Social Change in a Mississippi County*. 1194
Steenberg, Sven. *Vlasov*. Reviewed by George Fischer. 543
Stein, Arthur. *India and the Soviet Union: The Nehru Era*. Book Note by Alfred Fernbach. 231
Stein, Arthur. Review of S. P. Varma and K. P. Misra, eds., *Foreign Policies in South Asia*. 882
Stephens, Stephen V. *Communication* on Aiken and Alford "Community Structure and Innovation: The Case of Public Housing." (September 1970). 499
Stephenson, William. *The Play Theory of Mass Communication*. Reviewed by Timothy Alden Williams. 514
Stern, Alan J. Review of Luciano Brunelli et al., *La Presenza Sociale del PCI è della DC*. 1197
Stern, Robert W. *The Process of Opposition in India*. Reviewed by Stanley A. Kochanek. 1231
Stern, Robert W. Review of Baldev Raj Nayar, *National Communication and Language Policy in India*. 560
Stocking, George W. *Middle East Oil: A Study in Political and Economic Controversy*. Reviewed by L. Hirszowicz. 881
Strategie des Gleichgewichts: Deutsche Fried-

enspolitik und die Weltmächte. Helmut Schmidt. Reviewed by Joachim Hütter. 1245
Strayer, Joseph R. *On the Medieval Origins of the Modern State.* Reviewed by Antony J. Black. 1171
Strover, Anthony J. and J. Gregory Oswald, eds. *The Soviet Union and Latin America.* Reviewed by George I. Blanksten. 837
The Structure of Local Government. Samuel Humes and Eileen Martin. Reviewed by Francine F. Rabinovitz. 1212
Student Politics in Chile. Frank Bonilla and Myron Glazer. Reviewed by Ben G. Burnett. 822
Student Politics in France: A Study of the Union Nationale des Etudiants de France. A. Belden Fields. Reviewed by Bernard E. Brown. 542
Studies in International Adjudication. R. P. Anand. Reviewed by Leo Gross. 865
Studying Elite Political Culture: The Case of 'Ideology.' Robert D. Putman. 651
Sugarman, Stephen D., John E. Coons and William H. Clune. *Private Wealth and Public Education.* Reviewed by Earl F. Cheit. 798
Sukarno and the Struggle for Indonesian Independence. Bernhard Dahm. Reviewed by Ann Ruth Willner. 540
Sun Yat-sen and the Origins of the Chinese Revolution. Harold Z. Schiffrin. Reviewed by Shao-chuan Leng. 853
The Supreme Court and the Electoral Process. Richard Claude. Book Note by Peter G. Fish. 208
The Supreme Court and the Uses of History. Charles A. Miller. Reviewed by David Farrelly. 1190
Surkin, Marvin and Alan Wolfe, eds. *An End to Political Science: The Caucus Papers.* Reviewed by Vernon Van Dyke. 793
Sussmann, Leila A. Review of Arthur Liebman, *The Politics of Puerto Rican University Students.* 1220
Sweezy, Paul M. and Leo Huberman, eds. *Régis Debray and the Latin-American Revolution.* Reviewed by George I. Blanksten. 837
Swygard, Kline R. Book Note on Richard A. Falk, ed., *The Vietnam War and International Law, Volume 2.* 236
Syed, Anwar. Reviews of Walter Lippmann, *Early Writings* and *Men of Destiny.* 512

Tabb, William. *The Political Economy of the Black Ghetto.* Reviewed by Morris Levitt. 1176
Taborsky, Edward. Review of I. William Zartman, ed., *Czechoslovakia: Intervention and Impact.* 573
Tarrow, Sidney. *The Urban-Rural Cleavage in Political Involvement: The Case of France.* 341
Tauber, Kurt P. Review of John David Nagle, *The National Democratic Party: Right Radicalism in the Federal Republic of Germany.* 847
Taylor, Charles Lewis. Review of Kenneth Janda, *Data Processing: Applications to Political Research.* 1165
Taylor, Michael and Douglas W. Rae. *The Analysis of Political Cleavages.* Reviewed by Kenneth A. Shepsle. 790
Taylor, Michael and V. M. Herman, *Communication* on criticism of their "Party Systems and Government Stability." 1149
Taylor, Michael and V. M. Herman. *Party Systems and Government Stability.* 28
Taylor, Telford. *Nuremberg and Vietnam: An American Tragedy.* Reviewed by John H. E. Fried. 1257
Theories in Search of a Curve: A Contextual Interpretation of Left Vote. Adam Przeworski and Glaucio A. D. Soares. 51
Theory Construction, From Verbal to Mathematical Formulations. Hubert M. Blalock. Reviewed by Charles F. Cnudde. 1161
Thompson, Dennis F. *The Democratic Citizen: Social Science and Democratic Theory in the Twentieth Century.* Reviewed by John D. Lewis. 1172
Thompson, Kenneth W. Review of Robert G. Wesson, *The Imperial Order.* 1258
Thomson, Robert. *No Exit From Vietnam.* Reviewed by Edward Reynolds Wright. 1232
Tinder, Glenn. Review of Joel Feinberg, *Doing and Deserving: Essays in the Theory of Responsibility.* 788
Toma, Peter A., ed. *The Changing Face of Communism in Eastern Europe.* Reviewed by R. Barry Farrell. 1234
Toma, Peter A. Review of Norman J. G. Pounds, *Eastern Europe.* 562
Totten, George O. Book Note on Chitoshi Yanaga, *Big Business in Japanese Politics.* 233
Toward a More Responsible Two-Party System: Political Science, Policy Science, or Pseudo-Science? Evron M. Kirkpatrick. 965
Toward a National Urban Policy. Daniel P. Moynihan, ed. Reviewed by Eric C. Freund. 810
Toward a Rational Theory of Decentralization: Another View. Frank Levy and Edwin M. Truman. 172
Towards A Better Use of the Ocean. William

T. Burke. Reviewed by Wolfgang Friedmann. 578
Treaties and other International Agreements of the United States of America, 1776–1949, Vols. I–IV. Compiled under the direction of Charles Bevans. Book Note by Salo Engel. 234
Trends in Popular Support for the Wars in Korea and Vietnam. John E. Mueller. . . . 358
Truman, Edwin M. and Frank Levy. *Toward a Rational Theory of Decentralization: Another View.* 172
Truth and Power: Essays of a Decade, 1960–70. Hans J. Morgenthau. Reviewed by Wolfram F. Hanreider. 879
Tullis, F. LaMond. *Lord and Peasant in Peru: A Paradigm of Political and Social Change.* Reviewed by Carlos A. Astiz. . . 855
Tullock, Gordon. *Private Wants, Public Means: An Economic Analysis of the Desirable Scope of Government.* Reviewed by Mark Sproule-Jones. 515
Tung, William L. *International Law in an Organizing World.* Reviewed by Clifton E. Wilson. 583
Turmoil and Transition: Higher Education and Student Politics in India. Philip G. Altbach, ed., Reviewed by Irene A. Gilbert. 531
Turner, John E. and Robert T. Holt, eds. *The Methodology of Comparative Research.* Reviewed by G. Bingham Powell, Jr. 545
Turner, Julius. *Party and Constituency: Pressures on Congress* (Revised Edition by Edward V. Schneier, Jr.). Reviewed by Frank J. Sorauf. 817

Ullman, Joan C. Review of Charles W. Anderson, *The Political Economy of Modern Spain: Policy-Making in an Authoritarian System.* 1196
Understanding Modern China. Joseph M. Kitagawa, ed. Reviewed by Thomas P. Bernstein. 549
United States Policy Toward Latin America: A Study in Domestic and International Politics. E. Harrison Wagner. Reviewed by Bryce Wood. 591
The United States Soldier Between Two Wars. Jack D. Foner. Reviewed by M. D. Feld. 803
Unused Power. Stephen Horn. Reviewed by Barbara Hinckley. 1185
Upgrading the American Police. Charles B. Saunders, Jr. Reviewed by Samuel G. Chapman. 527
Urban Government for Metropolitan Lima. Allan G. Austin and Sherman Lewis. Reviewed by David Collier. 820
The Urban-Rural Cleavage in Political Involvement: The Case of France. Sidney Tarrow. 341
U.S. Corporate Investments in Taiwan. Jordan C. Schreiber. Reviewed by Alek A. Rozental. 1229
The USSR Arms the Third World: Case Studies in Soviet Foreign Policy. Uri Ra'anan. Reviewed by Robert G. Wesson. 562

Vallier, Ivan. *Catholicism, Social Control, and Modernization in Latin America.* Reviewed by Kalman H. Silvert. 1221
van den Berghe, Pierre L. Review of George W. Shepherd and Tilden J. LeMelle, eds., *Race Among Nations: A Conceptual Approach.* 845
Vandenbosch, Amry. Review of George W. Shepherd, Jr., *Nonaligned Black Africa: An International Subsystem.* 1255
VanDeVeer, Donald. *Oppenheim's Defense of Noncognitivism.* 1105
VanDeVeer, Donald. *Rejoinder to Oppenheim's "Comment."* 1117
Van Dyke, Vernon. Review of Marvin Surkin and Alan Wolfe, eds., *An End to Political Science: The Caucus Papers.* . . 793
Varma, S. P. and K. P. Misra, eds. *Foreign Policies in South Asia.* Reviewed by Arthur Stein. 882
Vieg, John A. Review of Michael Paul Rogin and John L. Shover, *Political Change in California: Critical Elections and Social Movements, 1890–1966.* 1192
The Vietnam War and International Law, Volume 2. Richard A. Falk, ed. Book Note by Kline R. Swygard. 236
Vincent, Jack E. *Predicting Voting Patterns in the General Assembly.* 471
Vines, Kenneth N. and Richard J. Richardson. *The Politics of Federal Courts: Lower Courts in the United States.* Reviewed by George F. Cole. 814
Vlasov. Sven Steenberg. Reviewed by George Fischer. 543
Vogel, Ezra. *Canton under Communism: Programs and Politics in a Provincial Capital, 1949–1968.* Reviewed by Chün-tu Hsüeh. 856
Völgyes, Iván. *The Hungarian Soviet Republic, 1919.* Reviewed by Bennett Kovrig. 857
Volin, Lazar. *A Century of Russian Agriculture: From Alexander II to Khrushchev.* Reviewed by Sidney I. Ploss. . . . 1235
von Beyme, Klaus. *Die Parlamentarischen Regierungssysteme in Europa.* Reviewed

by Karl Loewenstein. 858
Voting in the Security Council. Sydney D. Bailey. Reviewed by David A. Kay. 1243

Wagner, E. Harrison. *United States Policy Toward Latin America: A Study in Domestic and International Politics.* Reviewed by Bryce Wood. 591
Wallace, Paul. Book Note on Shanti Kothari and Ramashray Roy, *Relations Between Politicians and Administrators at the District Level.* 223
Waller, Derek J. Book Note on Jack Gray, ed., *Modern China's Search for a Political Form.* 226
Wars, Plots and Scandals in Post-War France. Philip M. Williams. Reviewed by Frank L. Wilson. 861
Waterbury, John. *The Commander of the Faithful: The Moroccan Political Elite—A Study of Segmented Politics.* Reviewed by Clement Henry Moore. 570
Webster, J. B. and A. A. Boahen with H. O. Idowu, *History of West Africa.* Reviewed by L. Franklin Blitz. 571
Webster, Michael G. Review of Jerome Levinson and Juan de Onís, *The Alliance that Lost its Way: A Critical Report on the Alliance for Progress.* 1250
Weidner, Edward W., ed. *Development Administration in Asia.* Reviewed by John T. Dorsey. 1237
Weinstein, Brian. Book Note on David and Marina Ottaway, *Algeria: The Politics of a Socialist Revolution.* 228
Welborn, David M. Review of Louis M. Kohlmeier, Jr., *The Regulators: Watchdog Agencies and the Public Interest.* 1186
Welch, William. *American Images of Soviet Foreign Policy.* Reviewed by Robert G. Wesson. 1193
Welch, William. Review of Morton Kaplan, *Macropolitics.* 510
Welch, William. Review of Sven Allard, *Russia and the Austrian State Treaty: A Case Study of Soviet Policy in Europe.* . 863
Wenden, D. J. Review of Robert J. Lieber, *British Politics and European Unity: Parties, Elites and Pressure Groups.* 1219
Wertheimer, Alan. Book Note on Felix E. Oppenheim, *Moral Principles in Political Philosophy.* 204
Wertheimer, Alan and Anthony de Crespigny, eds. *Contemporary Political Theory.* Reviewed by Wilfrid E. Rumble. 1162
Wesson, Robert G. *The Imperial Order.* Reviewed by Kenneth W. Thompson. 1258
Wesson, Robert G. Review of Uri Ra'anan, *The USSR Arms the Third World: Case Studies in Soviet Foreign Policy.* 562
Wesson, Robert G. Review of William Welch, *American Images of Soviet Foreign Policy.* 1193
Wheeler, Harvey. *Democracy in a Revolutionary Era: The Political Order Today.* Book note by Currin V. Shields. 206
Wheeler, Richard S. *The Politics of Pakistan: A Constitutional Quest.* Reviewed by George M. Platt. 1238
White, D. M. *Power and Intention.* 749
White, Irvin L. *Decision-Making for Space; Law and Politics in Air, Sea and Outer Space.* Reviewed by Carl Q. Christol. . . 1260
White, James W. *The Sōkagakkai and Mass Society.* Reviewed by James Allen Dator. 1240
White Racism: A Psycohistory. Joel Kovel. Review of Donald G. Baker. 1187
The White Savage: Racial Fantasies in the Postbellum South. Lawrence J. Friedman. Reviewed by Donald G. Baker. 1187
Why Men Rebel. Ted Robert Gurr. Reviewed by Ivo K. Feierabend. 194
Wilkie, James W. *The Mexican Revolution: Federal Expenditure and Social Change Since 1910.* Reviewed by Charles W. Anderson. 572
Williams, Philip M. *French Politicians and Elections, 1951–1969.* Reviewed by Vincent E. McHale. 859
Williams, Philip M. *Wars, Plots and Scandals in Post-War France.* Reviewed by Frank L. Wilson. 861
Williams, Timothy Alden. Review of William Stephenson, *The Play Theory of Mass Communication.* 514
Willis, F. Roy. *Italy Chooses Europe.* Reviewed by Norman Kogan. 1241
Willner, Ann Ruth. Review of Bernhard Dahm, *Sukarno and the Struggle for Indonesian Independence.* 540
Wills, Garry. *Nixon Agonistes: The Crisis of the Self-Made Man.* Reviewed by James MacGregor Burns. 818
Wilson, Clifton E. Reviews of William L. Tung, *International Law in an Organizing World;* and Edward Collins, Jr., ed., *International Law in a Changing World: Cases, Documents and Readings.* 583
Wilson, Frank L. Review of Philip M. Williams, *Wars, Plots and Scandals in Post-War France.* 861
Wirt, Frederick M. *Politics of Southern Equality: Law and Social Change in a Mississippi County.* Reviewed by Robert J. Steamer. 1194

5-9428
72-4398

Wilson, James Q. and Edward C. Banfield, *Political Ethos Revisited.* 1048
Witcover, Jules. *The Resurrection of Richard Nixon.* Reviewed by James Macgregor Burns. 818
Woetzel, Robert K. Review of W. J. Stankiewicz, ed., *In Defense of Soverignty.* 590
Wolfe, Alan and Marvin Surkin, eds. *An End to Political Science: The Caucus Papers.* Reviewed by Vernon Van Dyke. 793
Wolfe, Thomas W. *Soviet Power and Europe, 1945–1970.* Reviewed by Lawrence T. Caldwell. 884
Wolfinger, Raymond E. *Nondecisions and the Study of Local Politics.* 1063
Wolfinger, Raymond E. *Rejoinder to Frey's "Comment."* 1102
Wolfson, Murray. Review of Alexander Balinky, *Marx's Economics, Origin and Development.* 507
Woll, Peter. Review of Harold Seidman, *Politics, Position and Power.* 815
Wolters, Raymond. *Negroes and the Great Depression: The Problem of Economic Recovery.* Reviewed by John E. Crow. 528
Womack, John, Jr. *Zapata and the Mexican Revolution.* Book Note by Marvin Alisky. 232
Wood, Bryce. Review of E. Harrison Wagner, *United States Policy Toward Latin America: A Study in Domestic and International Politics.* 591
Wood, Stephen B. Book Note on Howard Jay Graham, *Everyman's Constitution. Historical Essays on the Fourteenth Amendment, the "Conspiracy Theory" and American Constitutionalism.* 214
The World Beyond the Charter in Historical Perspective: A Tentative Synthesis of Four Stages of World Organization. Wilfred Jenks. Reviewed by Robert W. Gregg. 874
Wright, Edward Reynolds. Review of Robert Thomson. *No Exit From Vietnam.* 1232
Wu, Yuan-li. Review of Jan S. Prybyla, *The Political Economy of Communist China.* 1226

Yanaga, Chitoshi. *Big Business in Japanese Politics.* Book note by George O. Totten. 233
Young, M. Crawford. Review of Wyatt MacGaffey, *Custom and Government in the Lower Congo.* 557

Zagoria, Donald S. *The Ecology of Peasant Communism in India.* 144
Zagoria, Donald S. Review of Isaac Deutscher, *Russia, China and the West.* 541
Zald, Mayer N. *Organizational Change: The Political Economy of the YMCA.* Reviewed by Ray E. Johnston. 201
Zapata and the Mexican Revolution. John Womack, Jr. Book Note by Marvin Alisky. 232
Zartman, I. William, *Communication* on review of Thompson's *Ghana's Foreign Policy 1957–1966* (March 1970). 190
Zartman, I. William, ed. *Czechoslovakia: Intervention and Impact.* Reviewed by Edward Taborsky. 573
Zoppo, Ciro E. Review of C. F. Barnaby, *Preventing the Spread of Nuclear Weapons.* 1243
Zucker, Norman L. Review of Ervin Birnbaum, *The Politics of Compromise: State and Religion in Israel.* 821

0187